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An Address
ON
THE RELATION OF PHYSIOLOGY TO MEDICINE.*

BY
THOMAS LEWIS, M.D., F.R.S.,
PHYSICIAN ON THE STAFF OF THE MEDICAL RESEARCH COUNCIL; PHYSICIAN, UNIVERSITY COLLEGE HOSPITAL, ETC.

THE relations between the science of physiology, which studies the normal functions of the animal body, and medicine, which studies and treats the human body in disease, are so intimate and manifold that the subject before us may be approached from many different points of view. When you, Mr. President, asked me if I would open this discussion, I felt some hesitation in accepting, knowing that I should speak from the standpoint of the physician. Although I have from time to time strayed into the proper realm of physiology, nevertheless I would ask you to remember while I address you that I am primarily a physician. It is for that reason that this paper emphasizes, not the standpoint of physiology, but the standpoint of medicine, for it is the welfare of medicine which I have firstly at heart.

Because the dependence, now, in the past, and in the future, of medicine upon physiology is not dwelt upon; because I do not speak, as I could speak, of the great and lasting benefits which physiology has conferred, and continues to confer, upon medicine as a science, and upon medicine as a healing art, it is to be hoped you will not think on this account that I am insufficiently alive to this relation of the two subjects. It could serve no useful purpose to come to physiologists and tell you what you all know, what is to every thinking man apparent—namely, that any knowledge which has reference to the normal functions of the animal body, however remote from practice that knowledge may seem at the time of its discovery, is sooner or later reflected or incorporated in medical thought and medical practice. Physiology, originally the offspring of medicine, has become to medicine in its old age a chief pillar of support. But, while this filial and noble aim of physiology has fulfilled, and continues to fulfil, its great purpose in nourishing and supporting its sire, it is fully recognized that physiology has other and equally noble aspirations. It seeks knowledge for its own sake, for the enlightenment of thought in other spheres. To take the view that physiology exists as the servant of medicine were as narrow as to countenance and encourage a complete servitude of human offspring to their parents. Each of these offspring may rightly claim a large measure of liberty. But as my own life interest is in medicine, so I may be forgiven if I rank myself with those who place the filial function first; I may be forgiven if, in recognizing that the two sciences, physiology and medicine, have mutual obligations to each other, I place, by reason of the same prejudice, the obligations of physiology well to the fore.

The obligations of physiology to medicine are twofold: to teach the student bent on the practice of medicine the normal functions of the human body, and in doing so to train him as an accurate observer and thinker; and to lay the foundation upon which the house of medical science shall erect itself. If you will, you may weld the two together and say that in its relation to medicine the function of physiology is to gather, classify, and spread knowledge and methods of studying the normal functions of the animal body. The first statement suits my purpose best, for it allows me to divide my thoughts into two chief channels, the teaching of medical students and the direction of physiological research—two channels, however, which should not diverge too far from one another. From both points of view the very closest association of physiology and medicine is necessary. The underlying thought has been to try and find means by which these sciences might be brought to closer and closer union.

Let us come at once to the pith of what I wish to say in regard to the teaching of physiology to medical students. In many respects it is essentially a controversial subject, and all the more suited therefore for discussion. The views expressed are personal views. It is my personal conviction that the pivot around which physiological teaching should play is human physiology; that human as opposed to animal physiology should form the central point of physiological teaching to medical men. That the physiological lessons are to confine themselves to the human body is not by any means what is advocated; such a course is clearly impossible, while by far the largest part of our knowledge of the functions of the human body is still derived analogically from observations upon the lower animals. The standpoint is that knowledge which has been obtained or is now obtainable by observation upon man himself should be constantly stressed, and that animal physiology should be taught where it supplements knowledge which is so obtained. It is of considerably greater consequence to the student of medicine that he should witness a given and normal phenomenon, or gather a given principle, by observation upon the human rather than upon the animal body; when, therefore, a phenomenon can be displayed to him both in the man and in the beast with equal accuracy, it should be displayed to him as it occurs in the former. When it cannot be displayed in man, or when it cannot be displayed with the same fine degree of certainty, then animal physiology should be called in. I would go further, and say that when a phenomenon can be displayed in man, even though it can be displayed less accurately than it can on the animal body, it should be demonstrated and stressed, not to the exclusion of the more precise demonstration on the lower vertebrate, but alongside of it, so that the two demonstrations may be evaluated and the two compared. There are several vital reasons why this should be the trend of modern physiological teaching to medical students. First and foremost is this reason: *that students in their most impressionable period would thus learn how to investigate the human body.* Not infrequently it is discovered in talking with physiologists that they but dimly realize the difficulties of this task, fail fully to appreciate how enormous is that barrier between physicians and the working organs—namely, the unbroken skin. Yet that barrier must and can be passed; the passage is vital to the present and future success of medical science and practice. The problem of what is happening in the human body is infinitely harder to solve than that which asks what is happening in the animal body, for in the former case our methods of examining and of observing are usually far more indirect. The student is taught to stimulate the vagus nerve in the frog and to note the effects; at the same time he should be taught to press upon the vagus in the carotid sheath of his fellow student, and to observe the consequent slowing of the heart beat. Physiologists have to face the fact that the large majority of the students who pass through their hands will encounter in their daily work problems far harder to solve than those with which they themselves deal in their laboratories. I speak from experience in saying that medical students, when they come to us in the teaching hospitals from the physiological laboratories, come to us insufficiently equipped in this respect. They have not yet learnt to handle a man; their senses of hearing, and especially those of touch, are not sufficiently developed; yet they are to handle men for the rest of their days; and my plea is that, with very few exceptions, they never will learn efficiently if they do not begin to learn while they work under your direction. They come to us in the hospitals after practising for two or more years relatively direct and conclusive methods; they come, many of them, with a rich knowledge of the functions of the body, with clear ideas of how problems of the animal body are approached and solved; they soon find by experience—often bitter experience—that most of these relatively exact methods which you employ are inapplicable to the study of living men; that many which are applicable are by their specialization soon beyond their reach; that far less direct, far cruder tests are of necessity used; that inferences are based upon premisses of a totally different quality. At the same time they are brought face to face with a mass of detailed knowledge of a new kind, they are asked to accept a new series of conclusions; upon many of these conclusions, it is true, their past teaching will bear; with many more their physiological teaching will not directly connect. The arduous work of the medical student in his final years is known to you; no one can pass through this last course, sifting and weighing a fraction

* Delivered in opening a discussion at the Physiological Section of the British Association at Cardiff, August 26th, 1920. (President, Joseph Barcroft, M.A., F.R.S.)

of what is taught him. The great bulk of what he hears in his hospital years must be taken on trust. It is by the force of these circumstances that the medical student invariably becomes empirical; and the measure of his empiricism is the measure in which he forgets his physiological teaching; this teaching becomes buried more or less in a mass of new experiences and methods. My contention is that his physiological training would not be lost to him in the large measure in which it indubitably is lost were the shock of change less, were his old experiences more in harmony with the new. It has been an intense interest of my own to watch for not a few years this stage of the medical student's career: it is his most critical time. I have closely watched a goodly number of men unusually well trained in physiology—I refer to men who have spent more time in physiology than the curriculum dictates—and have yet to see an example in which the deadening influence of this transition has not been conspicuously displayed upon the faculties of criticism and inference. It is an influence that I am conscious, from personal experience, that I have not escaped; it takes years to outlive it. You train a man to weigh with a nicely adjusted chemical balance; when you start him on his life's work a crude scale is alone available to him, and from the moment it comes into his hands he is set to weigh hard. He weighs and he forgets the old balance; in general he never fully appreciates the defects and limitations of the new; in time he acquires a mistaken confidence. These limitations, these defects, he should surely know beforehand; familiarity with those tools which are to be his as a workman, exact knowledge of their capacity to help him or to lead him astray, should be his from the very start. To train a man fully in the use of delicate instruments, to bring him then to his first real workshop and to set him to his bench of coarse and unknown tools, is to inflict upon him a thing which to many brings apathy, to some revulsion. Let him see and use his delicate instruments by all means, but let him clearly understand that these are in his hands, not only for the knowledge they bring, but also for comparison with those more simple devices which are also there for him to see and use, and which are to be eventually the weapons with which he will fight disease. In that way, and in that way only, so it seems to me, will he lastingly appreciate the limitations which circumstance and method impose upon him, and in that way will he assess the true values of his method. Such is, largely in metaphorical language, what I have in mind in pleading for human physiology. It will be said, perhaps, that the fault lies in medical teaching; in great part that is unquestionably true, but it is not to be forgotten that medical science labours before unusual obstacles; neither is it to be forgotten that teachers of medicine have themselves all passed through this same ordeal—an ordeal which leaves none unscathed; medical practice, driven by necessity, seizes those weapons which are alone at hand; the old trapper has a too easy-going confidence in the gun which has long been his only available companion. The confidence displayed by medical men in their methods is a chief fault in medical practice to-day; it is a personal opinion that that confidence can be modified, most materially and most quickly, in the physiological laboratory, by training students how to examine the human body and teaching them what physiological conclusions they may legitimately draw from such investigations; to show them side by side with such methods of investigation what can be done when, as with the lower animals, greater facilities are presented to them; to supplement conclusions drawn from experiment on other animals. An arrangement seems desirable whereby clinical material, illustrating clean experiments of disease which throw light on normal functions, should be brought into the hands of physiologists and their students. Such co-operation would form a valuable connecting link between physiology and clinical medicine, stimulating research and urging teaching in the direction which it should take—physiological teaching becoming more human, and medical teaching more in consonance with physiological thought. Such cases would illustrate instances of simple lesions of the nervous or cardio-vascular system or special senses. I have in mind clean transections or ablations and their effects which, when shown side by side with corresponding evidences from animal physiology, would form most impressive demonstrations. Demonstrations upon the human subjects are always impressive. Few physiological students who have heard the story, forget Alexis St. Martin. None would forget if they saw him in life. Salivary, gastric, or biliary fistulae are not so uncommon that they might not from time to time be shown to physiological students. Many of the patients who visit our hospitals because they present anomalies of the ductless glands might also enter your laboratories. I look forward to a time when all physiological laboratories which train medical students may be equipped with those instruments of examination which are to be found in teaching hospitals, for there are very few of these instruments which will not display in some measure the normal functions of organs of the human body. As an example I may cite the movements of the alimentary canal as these are shown by the *x*-ray tube.

But in regard to such instruments and devices I have some particular remarks to make.

There is, if I mistake not, a prevalent opinion that the medical man of the future is to be equipped as a physicist, as a chemist; that he is to have behind him an array of delicate apparatus and reagents with which exact measurement is to be made possible to him; and that in these directions the salvation of medicine is to be sought. If I am correct in my belief that such is a current view, then I would state emphatically that I believe it to be a profoundly erroneous view. It confuses two issues—the daily routine of the practitioner and the work of the special investigator. These stand apart: they always will stand apart. You cannot deal with masses of patients by using refined methods; repeatedly it has been tried; it always breaks down. The chief weapons of the general practitioner are to-day, and will remain, his own unaided senses; these are supplemented by a few simple devices. The healthiest trend of modern medicine—and let it be said at once that it is a trend which is anything but universal—is to dispense as far as possible with special methods of investigation in dealing with the everyday patient. The special method is for the investigator into pathological processes; it is not for the pure clinician. The use of special devices, of special tests, almost always leads one way: the devices and tests are employed by men who cannot have the training, cannot afford the time fully to understand, and a method imperfectly understood is, generally speaking, worse than useless. Take the method of electrocardiography; you will agree with me that it has added a very great deal to our knowledge of both the normal and perverted functions of the heart during recent years. I take this example because it is one with which no one in this country is more familiar than myself; and I say without hesitation in regard to it that so far as its employment as a direct aid to diagnosis, prognosis and treatment in individual patients is concerned, it has done far more harm than good. That is so because many of those who employ this instrument in clinical routine rely upon it to an extent far above its merits, and because it uses up time which would be spent more profitably in other ways: it fails especially because it attracts attention from other and far more important issues. The greater the number of such devices employed, the more the outstanding question recedes into the background. Is this man, as a whole, healthy or unhealthy? Be careful not to misinterpret me; I do not say that electrocardiography as a branch of medical science has harmed medicine more than it has served it; the reverse is decidedly true; my meaning is that in its application to the individual its effect is in general harmful, and will remain so. I have taken the galvanometer as my example; I have done so because it frees me from any possible suspicion of prejudice. The polygraph might have been used as an equally apposite illustration, and the same statements might truthfully have been made in regard to it. In my own clinical routine I gave up the use of both these instruments when, by their use, sufficient experience had been accumulated to identify the disorders of the heart which they display, by means of sight and touch. For that we ought constantly to strive. Let the knowledge of disease be built up by means of such instruments; let us sift from this knowledge what is of immediate and unquestionable service to the practice of medicine; let us then find how the special test may be supplanted, by methods perhaps not quite so accurate, but methods which can be at once and universally employed. Believe me, you will never see the day when the medical

practitioner will rightly interpret delicate records of the heart beat; but you will see the day when he will know the chief things which those records have taught us, and how he may utilize that knowledge in his daily work. A medical practitioner cannot hold a working knowledge of these refined methods; each has its growing complexity, each formulates innumerable rules and reservations; each is to him an abstruse method, applicable to but a small fraction of his patients. The plea for him is simplicity, not bewildering complexity. Treat polygraphy, treat electrocardiography as sources of knowledge and of generalization, and you may say that these methods have potentially fulfilled their great purpose; they have greatly simplified the diagnosis and treatment of heart disease; but treat them as parts of the medical man's equipment and you will load further what is already overloaded. It is eight years since, in the preface to my small book on *Clinical Disorders of the Heart*—a book describing how varieties of disordered heart action may be recognized by simple tests—I first expressed these ideas; they have since grown towards convictions, and, as you are aware, have found a powerful advocate in Sir James Mackenzie. Medical practice will grow more scientific as it becomes more and more simple—as the practitioner grasps great and fundamental truths in their relation to common diseases; it will not grow scientific in proportion to the mass of detail—impressive enough, maybe—which he is able to pack into the compass of his brains. The place of these refined methods is the laboratory and the teaching hospital. In the teaching hospital they are there so that the student may learn how knowledge is acquired. I have never, as a teacher of medicine, taught these methods to undergraduate students; neither will I do so. It is for them to use their fingers and their sight, but the instruments are there to check such observations, to show what can, and equally what cannot be done; they are there so that the student may see how the knowledge which he is to use is won. We have to teach our students not only how to handle disease, but also upon what the knowledge we impart is founded.

The second reason which is urged for a greater concentration upon human physiology in teaching is of a different, though hardly less important, character. Medical men are insufficiently conversant with what constitutes normality, or the range of normality, in the human being. They regard as pathological many phenomena which are in reality not only compatible with health, but are actually physiological. It is of much consequence that this argument should be brought home, and it may be done by means of a few notable examples. I take these from that sphere with which I am most fully acquainted by personal experience—the sphere which studies the circulation. They could be multiplied, and many others of the same kind could be cited from other branches of study, but these will suffice. Take a very simple case: Stroke the back of a normal healthy young subject and watch the vascular reaction in the skin; the line pressed and the surrounding skin displays after a while a red line; maybe it soon becomes surrounded by an area of blanching, maybe a line of blanching alone appears. These reactions, perfectly healthy reactions as they are, have been used, are still used, and will be used for many years to come, as important signs of particular and serious disease. Take the effect of respiration upon the human pulse. If the pulse falls away in strength in inspiration—*a fortiori*, if it disappears—it is held to be a sign of serious mischief in the pericardium, or of some other grave disorder; actually both signs are compatible with—nay, are common in—perfect health; the normal waxing and waning of the pulse rate in natural breathing has been responsible, and is still responsible, for the enforced invalidism of hundreds of young children. Normal variations in the amplitude of heart sounds, the presence of those murmurs which almost always accompany an accelerated action of the healthy young man's heart, send hundreds to their beds or couches annually; the same normal phenomena kept very many hundreds of strong, able bodied men from their proper stations in the time of the great war. These and similar variations in normality are insufficiently taught in our physiological schools, where they can be most convincingly taught. Any departures from a single physiological standard come inevitably to be regarded as abnormal; busy medical men have no time to seek for and to study controls; for the most part they leave the physiological study of man when they leave the physiological laboratory and lecture theatre. The result is that the knowledge of normality in man is often far more elementary than it has business to be.

Take the physiology of exercise, the measured amount of work which healthy men of different ages and of different training can do in a given time, or the reactions of pulse and respiration which a simple test exercise should call forth in his body. That simple physiological knowledge is vital to proper medical practice, yet I have no hesitation in saying that only a few medical men have this requisite knowledge to-day. That surely is a reflection on the method, if not of physiological then of our combined teaching. It is sad to think that medical men should fail to recognize a sick man, or should call a healthy man sick, because they lack full knowledge of healthy reactions; yet these are common daily blunders. When I look back over the work upon which I have been engaged in the last eighteen years since I left the physiological laboratory I discover that a large fraction of my time and energy has gone in discovering phenomena of health which hitherto I had learned to regard as manifestations of disease. To teach the principles of their science, to teach men to observe accurately and to think critically are wise aspirations of physiologists. A man may be given a scientific training in chemistry, but if practical chemistry is to occupy little or no part of his active life, much of the virtue of the training goes out of it. The argument—it is no new argument—is that physiology as it is now taught to medical students is not sufficiently correlated to medical practice: it is not human enough. If, in illustrating a principle or in impressing accuracy of observation, there is a choice of subject—and selection has become imperative with the rapid expansion of your science—it is better to choose an illustration which may be followed by a corresponding demonstration upon man. Do medical men show unusual discrimination when they come to weigh the pros and cons of a new problem? Are their explanations weighed in the critical spirit which their physiological teachers have tried to instil into them? Are they unusually acute in assessing the value of evidence? I do not answer these questions negatively, but lack the assurance to answer them affirmatively. If they are to be answered negatively, does it not mean that there is some weakness in physiological teaching, for physiology is the chief philosophical subject which they study? And may not this defect, if it exists, in part at least result because they learn to philosophize upon questions which are soon forgotten in the daily routine of practice? It matters little to a practitioner whether Ludwig or Heidenhain was in the right; he does not base his treatment on one or other view. An account of many such controversies, and especially those which have led as yet to no final conclusion, might well be replaced by a historical account of those problems which are more vital to the daily needs of medicine, and these problems might equally illustrate processes of argument and thought. With purely physiological hypotheses, we, as pathological investigators, have little or no business. I use the term hypothesis in its true meaning—an assumption intended as a temporary explanation of observations, and put forward in the hope that with extended investigation such hypothesis may harden into conclusion. A published hypothesis is usually in essence a redoubt built by its conceiver; he and his party labour to strengthen it; but its very nature invites, and sooner or later provokes, an attack, and, as with redoubts, it is but rarely impregnable. The attack and counter-attack may be and often are prolific of discovery, whether the redoubt holds or falls. It is at once conceded that physiological hypothesis is essential to the physiologist; it has not the same value to medicine, it is often dangerous when introduced to medicine. The observations and conclusions of physiology are indispensable to us; but too often, from the inability to weigh evidence, hypothesis and conclusions are confused, and the former becomes the basis of pathological speculation. This is a widespread and regrettable habit amongst pathologists and clinicians. Who builds a house upon an exposed and threatened redoubt builds unwisely. The pathologist or clinician who founds his own assumptions on the assumptions of physiology, builds to see the complete collapse of his edifice. He who founds his assumptions on the observations and conclusions of physiology may still see his house totter, but the foundation will remain secure, and, happily, some parts of the superstructure may remain intact if solidly laid.

In turning more particularly to questions of original research, I again stress the need of more closely uniting physiological and pathological thought; in that way only will the student of medical science remain conversant with physiological conclusion, and be in a position to divide it sharply from hypothesis. While it would be good to see our anatomists in our *post-mortem* rooms, it would equally be good to see our physiologists more often in our wards. It is scarcely to be doubted that such visits would lead to very fruitful investigations, would correct misconceptions on both sides. That is but one way in which constant touch might be attained. In the "Physiological Society" we have possessed in the past a connecting link, though it be but an occasional link; to this society the leaders of pathological research, many leaders of clinical research, have belonged; in times past they have provoked some of your most interesting and fruitful discussions, stimulated or produced many important physiological researches. But as physiology becomes ever purer (more physical and more chemical) it becomes more and more difficult for men engaged in clinical investigation to feel at home at those meetings. It is no answer to say that medicine should follow the same lines, go back to the simple physical and chemical basis; it cannot do so at the same rate; medicine is inevitably preoccupied with end problems, and cannot escape from the expedients and opportunisms which its constant association with very sick people forces upon it. Here arises a tendency for the pure physiologist and purely clinical investigator to drift apart. They must be kept together, not only for the sake of medicine, but also for that of physiology. The breach has been partly closed by contact at the meetings of the admirable society to which I have referred; it has also been closed in part by a group of men who have devoted much of their energy to this task. I refer to those investigators who have devoted themselves to what is now often termed "experimental medicine." Taking for their field of study diseases of particular organs or systems of organs, they have sought to bring the physiological and pathological side of their subject into harmony. Horsley, Ferrier, Head, Garrod, Mackenzie, and many others have been recently or are still amongst this group of active workers. The group of men has been formed from those practising medicine or surgery, notably from the ranks of neurologists; students of applied physiology have been drawn too exclusively from the ranks of those who are primarily clinicians, and insufficiently, as I think, from the ranks of trained physiologists. A chief reason why this has been so in the past is that "experimental medicine," as it is often called, has offered no immediate chance of livelihood. My view is that the number of physiologists who by contact with clinical problems are inspired to work upon problems of applied physiology is still insufficient; the war period, during which many physiologists were so engaged, showed the fruitfulness of such contact. Medical research is now nourished on a new basis in this country, and it looks as if the link, "experimental medicine," may, when it is fully forged under the Medical Research Council, go far to fill what is still an obvious want—namely, the closer co-operation of all research work in the clinical and pathological fields with contemporary physiological thought.

In the discussion that followed, Professor H. E. ROAF thought some alteration in the teaching of physiology was needed. He was impressed by the plea for simplification, so as to give the student less detail and more fundamental principle. While agreeing that as much as possible should be illustrated on the human subject, he feared there was a danger in too much "clinical" physiology. The clinical applications were largely a subject for the clinical units and experimental medicine; the physiologist should inculcate a sound knowledge of principles rather than illustrate a multiplicity of technical instruments. Professor SWALE VINCENT found himself very generally in agreement with the views expressed. Dr. Lewis had pointed out certain defects in the current methods of teaching physiology to medical students. Most of the physiologists present could point to a hundred other defects; they were nearly all due to lack of funds. Extension of the teaching of human physiology by observations on the students themselves was the more desirable, and indeed necessary, since students had little opportunity for any other kind of mammalian work, their observations usually being confined to the frog.

EIGHTY-EIGHTH ANNUAL MEETING

OF THE

British Medical Association.

Held at Cambridge, June 30th to July 2nd.

PROCEEDINGS OF SECTIONS.

SECTION OF SURGERY.

Sir GEORGE H. MAKINS, G.C.M.G., C.B., P.R.C.S., President.

DISCUSSION ON THE END-RESULTS OF INJURIES TO THE PERIPHERAL NERVES TREATED BY OPERATION.

OPENING PAPERS.

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I PROPOSE to place before you some general considerations, and to endeavour, as far as I can, to give you a summary of recent work upon the subject with which we are concerned. I do not intend to trouble you with personal experiences, first, because I do not regard this as the proper function of the introducer of such a discussion; and, secondly, because I have been abroad throughout the greater part of the war, and thus I have not had so large a number of personal operation cases as those who have been working in base hospitals in this country.

In civil life injuries to the peripheral nerves, although by no means rare, do not occur in sufficient numbers to allow of the collective investigation which they receive in war. Hence much of our earlier clinical knowledge was founded upon the work of Weir Mitchell during the American Civil War, and since that time research has been conducted mainly either on experimental or pathological lines or on the limited clinical opportunities of a few observers. The European war has again supplied us with a large amount of material, which has been thoroughly studied, and which has been treated under the most modern surgical conditions. Great numbers of these cases have been submitted to the operation of secondary suture, and not a few to that of grafting, so that we may hope that it will be possible ultimately to express almost in a statistical form the end-results which have thus been obtained. This day has, however, not yet arrived, and a long period of time and a careful series of observations by neurologists and anatomists will be required for full investigation. We are thus not yet able to give a perfect picture of end-results; all that can be done is to collate some of the more complete records and to formulate what may perhaps be regarded as an interim report.

Before doing so, however, I would like to refer briefly to a few preliminary considerations which are of importance in directing our treatment, and which indicate the type of end-result which we may legitimately hope to obtain. It may now be agreed that the regeneration of a peripheral nerve is in all cases accomplished by the downgrowth of axons from the proximal end into channels or sheaths awaiting them at the peripheral end. The proximal axon is connected with a sensory or motor neurone, while the peripheral sheath leads to or is connected with a terminal organ, either motor or sensory. It must therefore be recognized that, apart altogether from *anatomical* continuity, repair which may lead to a good *functional* result requires that a sufficient number of axons shall enter into a sufficient number of peripheral sheaths which are capable of leading them in the right direction. Thus, for example, motor axons entering channels connected with sensory terminals are probably lost for ever, and so with afferent axons which become connected with muscle endings. So far as I know, the union of a purely motor with a purely sensory nerve is not followed by any physiological connexion whatever.

It is clear, therefore, that motor axons must enter motor channels, and afferent axons sensory channels. Not only

is this the case, but it is important that as far as possible the down-growing axons should enter the identical channels for which they were originally intended. Thus, if the spinal motor celis for, let us say, the extensor longus pollicis become connected with the peripheral channels for the brachialis anticus which lie in the same nerve, physiological confusion will result, and the spinal cord will not be able to produce the movements which the brain demands. Similarly, irregular connexion of afferent nerves with terminal organs in the skin or elsewhere for which they were not intended will necessarily lead to more or less of chaos in the interpretation of afferent impulses. No doubt errors of this type can be to some extent corrected by means of education, but such *re-education* will be necessary in the great majority of cases of nerve suture, and it is especially necessary in connexion with suture after the excision of large cicatrices from complicated junctions such as the brachial plexus.

We are thus brought face to face with the question of shunting, or, as the French call it, "aiguillage," of axons into peripheral channels. Shunting may be gross or macroscopic, as when the proximal end of the hypoglossal or spinal accessory nerve is attached to the distal end of the facial, in which case a limited physiological result may be obtained. Motor axons here enter channels for which they were not intended, and the resulting movements are confused and imperfect, but they can and do occur. The gross shunting of a purely sensory on to a purely motor nerve or vice versa can produce no result at all. The operation which has been adopted in the past of implanting the two ends of a cut nerve into an intact adjacent nerve ignores altogether the essential linking up of original channels, and has in practice entirely failed. In this connexion I would urge that the word anastomosis, which is founded on a false analogy between nerves and hollow channels such as blood vessels, should be entirely abandoned. Anastomosis in the real sense of the term never occurs, and the use of the word is apt to lead to confusion of thought.

Apart, however, from gross or macroscopic shunting, histological shunting probably occurs in the vast majority of operations. In the case of end-to-end suture every endeavour is made to avoid errors in shunting by preventing rotation of the approximated ends, but even with this precaution some error probably always occurs, owing to the nerve fibres running in a spiral course, so that, however perfect the anatomical adjustment, the minute apposition is not likely completely to restore the axons to their original channels. To a much greater extent this difficulty arises when a graft has been interpolated. In such cases the axons of the proximal end are expected to enter the neutral channels of the graft, to pass through them, and then, so far as possible, to re-enter their proper channels at the other end of the graft. It is clear that under such conditions the prospects of correct shunting will be at least very greatly diminished, and that, even if good anatomical results can be obtained, the physiological results are likely to be most imperfect.

Before proceeding to our analysis, we have one other consideration of importance in judging of an end-result. Such a result can be anatomically good in the sense that nerve continuity is restored; it may be physiologically or neurologically satisfactory to the extent that on stimulating the nerve every muscle with which it is connected may be made to contract, and the whole of its cutaneous distribution may present at least some sensation, but we have also to consider what I may call the *economic* value of the end-result. A result which is neurologically good may be economically poor, in the sense that the patient is unable to use the affected part as he ought to do. Such defect may be due to psychical conditions, to the secondary changes of muscular atrophy or of arthritis, or possibly to a form of ataxia in which, while all muscles are capable of action, they can only be properly used while their action is under visual control. This local ataxia, which may render economically imperfect an otherwise excellent limb, is probably due to want of restoration of the deep afferent fibres by which are judged and controlled perfect muscular action.

Alternatively and more fortunately, the economic result may be better than the physiological result. Thus in cases of injury to the ulnar nerve with well defined symptoms, those who do not require the finer movements of the hand may find little practical inconvenience, and even a complete lesion of this nerve may exist without recognition by the patient. Again, vicarious movements may admirably replace those of lost muscles. How varied such vicarious movements may be, and to what a degree patients may conceal paralysis which actually exists, is well illustrated in a recent paper by Forrester-Brown.[1] Vicarious movements and substituted movements of this type have an enormous economic value, but they constitute a veritable pitfall, in the estimation of neurological results, for those who are not constantly upon their guard or not thoroughly familiar with the possibilities of misreading end-results. The importance of their consideration is illustrated by the fact that I have personal knowledge of cases reported by good observers as "cured" in which there is no neurological evidence whatever of regeneration; and it is never possible to rely upon the statements of patients as evidence that recovery has occurred after operation.

With these unduly long introductory remarks, we will now turn to some of the more extensive series of reported results.

Of 108 personal cases of suture or grafting analysed by Tinel,[2] 14 were complete failures, 22, or say 20 per cent., showed practically complete recovery, and the remainder were regarded as improved.

Daw[3] says that 40 per cent. of his cases showed signs of returning motor power after seven months.

Sherren[4] found that of 21 cases of secondary suture, some return of motor power took place in all, but that it was never complete.

Kennedy,[5] examining after a long period cases occurring in civil life, claims that 73 per cent. of all sutures were completely successful.

Dumas[6] investigated 115 personal cases of operation for war wounds of one nerve only, the musculo-spiral; but it must not be forgotten that for several reasons this yields better results than any other nerve. He claims evidence of recovery in 42 per cent. of his cases, and of some regeneration in 65.2 per cent., the remaining 35 per cent. being presumably entire failures.

Joyce[7] found in 36 cases of secondary suture 8 recoveries, 6 with considerable improvement, 8 with some improvement, and no result in 14.

Souttar and Twining[8] found that of 61 cases of suture 7 recovered, 32 were recovering at the time of report, 7 were doubtful, 2 were failures, and 13 were not traced; but their results, like those of Joyce, would probably be improved by a later report, as in many cases the time elapsed had not been sufficiently long.

Adson[9] reports the results of operation on 41 cases, four-fifths of which were end-to-end suture and the remainder plastic operations. Of 30 cases traced "the average amount of improvement obtained was 58 per cent. return of the sensory, 62 per cent. return of the motor, and 67 per cent. return of the trophic function"; 73.1 per cent. showed improvement.

These figures, which are by no means easy to compare, leave upon my mind the impression that secondary nerve suture gives very good results in somewhere between one-third and two-thirds of all cases, but the most careful study of original articles, as well as my personal experience, leads me to believe that a perfect neurological recovery is rarely if ever obtained. Permanent disability may be masked in various ways, but is almost invariably to be detected if carefully searched for. Complete failures are by no means uncommon, and must be looked for in a very definite percentage of even the best operations. Such appear to be the general results in the case of secondary suture.

In the case of grafting, as we have already seen, much less is to be hoped for. Certain isolated cases are reported in which brilliant results are claimed, and it is, of course, possible that the anatomico-pathological difficulties to which we have referred may occasionally be overcome, but there is certainly as yet no large number of good results obtained from grafting. Personally I have no experience at all of this operation, and it will, I think, be found that the greater the experience of end-to-end suture, the less often the need for grafting will be found to arise. So much can be done by bold operating, by extensive exposures, by thorough stretching of nerve trunks, by alteration of their anatomical course into a shorter bed, and by the operation of stretching in two stages, that grafting will always be, and should always be, a last resort. I have yet to see personally any cases in which it has been a success. I have seen cases in which it has been reported as successful, but which to my mind did not carry conviction. I hope, however, that there are those present who will be able to establish a claim to have obtained results more fortunate than I have met with.

Coming now to a few details, I should like to call your attention to a number of facts now well established which materially influence our prognosis in the case of different nerves and different levels of these nerves. Nerves which supply large muscles, and especially muscles of homogeneous type, such as the musculo-spiral, give much better results than those whose muscular distribution is small and complicated, such as the median and ulnar. Nerves also which are mainly motor or mainly sensory are more amenable to treatment than those whose function is more complicated. In both these cases no doubt the ill effects of bad shunting bear more heavily upon the more complicated trunks. Dual lesions are more serious than single ones, so that a combined injury of the median and ulnar nerves is susceptible of very little improvement indeed.

The time required for recovery is very great. On the basis of what is known as Tinel's sign the downgrowth of axons occurs at the rate of about 1 mm. per diem, which would require, say, 200 days for a fibre to grow from the wrist to the tip of the index finger, or two years from the middle of the thigh to the foot. The slow return of epicritic sensation, even after a perfect primary suture, is admirably indicated by Sherren's operation upon Head. An ophthalmic surgeon under my care some years ago divided the digital branches of his left, middle, and ring fingers near the web by a cut from a piece of glass, and did not obtain perfect tactile sense for eighteen months or two years. On the other hand, the rate of recovery is not in such direct proportion to the length of nerve to be traversed as was formerly believed, and probably a large share of the time actually occupied is required for the linking up of axons with end organs, and for re-education. In this connexion Stopford[10] has some most interesting figures; analysing 58 cases of suture of the musculo-spiral nerve in the lower, middle, and upper thirds of the arm, he finds that the supinator longus begins to recover after about four, five, and seven months respectively, other muscles showing similar differences; so that it takes only about three months longer for recovery in a section close to the axilla than in the case of a section quite near to the origin of the branch for the supinator longus.

It would also appear from the work of Stopford, who has most carefully tested 271 cases of nerve suture, that recovery is more perfect the nearer the suture is to the spinal cord, which may be due to its closer connexion with its trophic centre, or to the greater ease of surgical operation upon thicker trunks.

Before concluding, I should like to refer to the so far unpublished results obtained at the Grangethorpe Military Hospital in Manchester, staffed by old students of my own with whose work I am thoroughly familiar. Most of the operating work here has been performed by H. Platt, and the results have been checked by Professor Stopford from a purely anatomical and neurological point of view. In a recent communication to the Royal Society of Medicine Stopford states that:

> In no cases of secondary suture can the result be described as perfect. At the best there has been a perceptible difference in power when comparing the two sides, and at least an incomplete recovery of epicritic sensibility, even three and a half years after the date of the suture. The recovery of sensation has been particularly disappointing.

Of sutures in the upper arm, failures amounted to 12 per cent., and in the forearm to 24 per cent. At both levels the recovery of voluntary power has been much less over muscles supplied by the nerve in the distal than in the proximal part of the limb. On the whole, it may be concluded that a good functional result is to be expected after secondary suture of the musculo-spiral nerve. The extensors of the wrist usually regain full power, the extensor communis digitorum recovers satisfactorily, but the extensors of the index finger and thumb are much less perfect. As regards the median nerve, in quite a large proportion only incomplete recovery of protopathic sensibility has occurred, and voluntary power in the abductor brevis pollicis has only been found in 45 per cent. The ulnar nerve is particularly disappointing, and the recovery of voluntary power in the small intrinsic muscles of the hand was exceedingly poor. On the other hand, from the economic point of view it is interesting to note that many of these injuries to the median and ulnar nerves did not prevent the patients from obtaining employment. I do not propose to weary you with details in reference to the nerves of the lower limbs.

These remarks apply to cases which date back for a period of three years; still further lapse of time may improve them, but will also tend to confuse the issue by permitting of improved vicarious function. As they stand they are, I believe, a perfectly fair expression of the best efforts of the surgery of to-day.

As to the dates at which recovery, if any, can be expected after the operation of nerve grafting we have no reliable statistics, and the comparatively few records of such recovery which will even bear investigation are not numerous enough for generalization.

I should like to conclude with a word upon the results of treatment of compression of nerves. Here my own experience has been very considerable, and almost, if not quite, uniformly successful. I have in the past had many examples of operation for compression of nerves, and I do not think that, however grave the symptoms, I have ever failed to obtain recovery which was at least economically complete; I have not the details to show how far they were neurologically perfect. My personal experience thus differs sharply from that of Kennedy, who found that neurolysis yielded slightly less good results than nerve section and suture. I have only one reservation to make: after the removal of pressure by cervical ribs it is most rare for atrophy of muscles to be entirely repaired; this I am inclined to attribute to the fact that a chronic neuritis has followed the original pressure lesion, and that such neuritis leaves indelible changes which are rarely found in traumatic cases of pure compression.

You may think that I have not painted the picture in sufficiently rosy colours; but you will generally find that the greatest optimism is that of the man who records a few remarkable cases—often a single case. After many years, I can only say that if a surgeon obtains 50 per cent. of really good economic recoveries after secondary nerve suture he is doing well; if he obtains 75 per cent. of such recoveries which will bear investigation, his work is both brilliant and fortunate. I trust that the majority of those present are in this class. On the other hand, you will notice that of death rates and of operation risks or disasters I have not said one word, because under modern conditions such things are and should be for all practical purposes unknown.

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NERVES implicated in war wounds rarely present the comparatively simple problems of recovery raised by the experimental division of a nerve, or even by the common "cut wrist" accident where a nerve is cleanly divided by a fragment of glass. In war wounds nerves are bruised and lacerated; the damage caused by a missile of high velocity is not limited to the immediate neighbourhood of the injury; surrounding structures are often extensively damaged; a main artery may be obliterated. Septic infection, often severe and prolonged, delays operative repair of the nerve, prejudices its chances of regeneration, and often prevents the early application of those forms of mechanical and physical treatment which are essential for preserving nutrition and preventing stiffness of joints and tendons.

When we take into consideration all these adverse conditions it is surprising, not that failures are met with, but that good results are so often obtained. There are two main groups of cases in which surgical treatment is called for—namely, those which require operative measures for the *restoration of conductivity*, and those which demand surgical interference on account of *disordered function*. In the one group the natural activities of the nerve are in abeyance; in the other phenomena of abnormal activity are displayed.

1. So far as the *repair* of injured nerves is concerned, I shall confine my remarks to the commonest class of case with which the surgeon has to deal—namely, that of the nerve so severely damaged as to require resection and suture, leaving aside those cases in which it has had merely to be liberated from a constricting scar, as well as those where partial resection with suture and other means have been employed. It is within the experience of all that function is restored both more rapidly and more completely when the interruption of conductivity is due to some cause such as compression, the recovery from which does not necessitate regeneration. Further, it is well known that when a nerve has been bruised or crushed and its sheath has remained intact, recovery is more rapid and more complete than after division and suture.

In considering the results of operative treatment for divided nerves we should, I think, confine our attention to the cases of direct end-to-end suture, for, as far as I am aware, this is the only means whereby conductivity can be restored. Fortunately the cases are extremely rare in which this cannot be accomplished. Operative procedures based upon principles which are at variance with the known facts of regeneration, such as "flap operations" and "lateral implantations," need scarcely be considered. I have had the opportunity of examining two cases in which, some six months previously, the proximal and distal ends of a divided ulnar nerve had been implanted laterally into the intact median. The proximal end showed an ordinary terminal neuroma attached to the median nerve, which fortunately had only been slightly damaged. The distal end contained no axis-cylinders.

Nerve grafting, or the bridging of a gap between the ends of a divided nerve, has no practical value. It has excited much interest, and has been the subject of a great deal of experimental work, more particularly by Nageotte. So far as I am aware, no useful result has, in the human subject, ever been secured by means of nerve grafting, although in a few cases there is some evidence to suggest that nerve fibres have succeeded in traversing a short graft.

In reviewing results we must suppose that the best technique has been employed; that no coarse fixation stitch has been passed through the substance of the nerve, and that interrupted sutures of the finest and least irritating material have been used to join the cut edges of the sheath; that the surfaces have not been crushed together, but that a millimetre gap has been left between them; that no foreign substance has been wrapped round the junction; and that the wound has healed without suppuration. One cannot but believe that the better the technique employed, and the more perfect the operative details, the better are the results to be expected. Yet one must confess to having had relative failures after what seemed to be satisfactory operations, and successes where the operations were certainly very far from perfect. Good results depend upon the happy combination of so many factors that it is not easy to explain such divergences. I believe the most important determining factor to be the degree of interstitial change which has taken place in the nerve as the result of septic infection. A second cause doubtless lies in the neutralization of axons which must result from motor fibres growing down sensory paths, and vice versa.

In speaking of results I shall not attempt to distinguish between primary and secondary suture. Whilst the great majority of cases are naturally those of secondary or late suture, a certain number of instances of early or primary suture have been observed. No evidence has been forthcoming that regeneration takes place either more rapidly or more completely in one than in the other. I take it that no one now believes in what was once known as the "primary union of nerves" with "immediate return of function"—a subject over which a good deal of ink has been spilt in the past. Once the peripheral end has undergone the normal processes of degeneration and of regeneration, it remains ready to receive new axons for an indefinite period. In the absence of sepsis it does not become fibrous. As far as regeneration of the nerve is concerned, secondary and primary unions have an equal chance. Inasmuch, however, as the wounds in which primary suture was performed had usually been treated early and radically, and had healed quickly, the handicap of prolonged suppuration and immobilization was avoided. It might therefore be expected that, other things being equal, such cases would give functionally better results. This has to some extent been borne out by the few cases which I have observed.

That a nerve which has been divided and sutured will regenerate and become capable of conducting impulses in both directions is, of course, within the experience of everyone. What we are now concerned with is to what degree of perfection such recovery may reach, and what in general are the prospects before a patient after the suture of a divided nerve.

In so far as the nerve injuries of the late war are concerned, we are not yet, I think, justified in using the term "end-results," at least on any extensive scale. It is true that the restoration of function in a good many instances after suture of the musculo-spiral and external popliteal nerves has already been so complete that in those cases it may be regarded as an end-result; but in the majority it cannot definitely be said that a stationary condition has been arrived at, even three or four years after the operation. It is very striking that many patients will, years after their operation, maintain that their limbs are continuing to improve. I should therefore at present prefer to use the term "late results" rather than "end-results."

What are the criteria by which these results are to be judged? We may, on the one hand, take as our criterion the *condition of the nerve* itself as evidenced by its ability to conduct impulses to or from the periphery. We may test the recovery of the neuro-muscular mechanism by examining the electrical reactions, or the power of voluntary contraction, in the muscles supplied by the regenerated nerve; and we may test the different forms of sensibility in its area of distribution. The application of these tests and the interpretation of the results are not simple matters to be disposed of in the course of a single examination. It is as important to make careful and repeated examinations during the period of recovery as during that probationary period which must often elapse before an operation is determined upon. Inferences from inaccurate observations are greatly to be deprecated, as tending to give false impressions of the results of nerve suture. It would be unnecessary to labour this point were it not that cases are published from time to time which grossly misrepresent the situation. Let me give an example: An account of a case appeared, in the medical press, of a gap in a median nerve having been bridged by a graft, after which, it was alleged, a certain degree of recovery had taken place. The case was advanced to support a plea for the more frequent use of nerve grafts. The evidences of alleged recovery were (1) that the patient could feel over a large area of the palm of the hand and fingers anatomically supplied by the median nerve; and (2) that he could abduct and oppose his thumb. I and several others had the opportunity of examining this patient, whose hand presented all the characteristic effects of complete division of the median nerve. There was no sign whatever even of any commencement of recovery. The area of skin over which pain and touch could be appreciated corresponded with the normal full supply of the ulnar nerve; the movements attributed to recovery of the median nerve were produced by other muscles.

Leaving aside gross errors of this kind, it is still easy to be deceived by trick movements into believing that recovery is taking place in the nerve. As a rule evidence of the commencement of motor and sensory recovery can be obtained at about the same time, though the rate of recovery in the two cases differs. If, therefore, there appears to be return of voluntary power in the absence of any sign whatever of sensory return, trick movements should be suspected.

The *second criterion* by which results may be judged is that of function. The regeneration of the nerve and the recovery of functional utility of the limb as a whole are two things which must not be confused. Unless the criteria are specified, the mere tabulation of results into good and bad, or satisfactory and unsatisfactory, serves no useful purpose, and may be very misleading. If they are grouped into good and bad according to the evidences of regeneration alone, and nothing is said about the functional utility of the limb, we may easily gain too rosy a view of them.

If, on the other hand, function alone is regarded, it is easy to be deceived into thinking that a nerve has regenerated well in a case where, in fact, it has done so very imperfectly or even not at all. When a patient sets himself to use the injured limb he concentrates his

attention upon bringing about the required movement, and if he cannot attain his object by means of the muscles which normally effect that movement he will learn to employ other muscles for the purpose, developing what are perhaps rather slightingly termed "trick movements." I have seen a patient with complete and irremediable loss of an ulnar nerve whose hand would easily pass as normal when looked at superficially and when judged only by its functional utility. He was, in fact, passed by a medical board as a perfect result after operation upon the ulnar nerve.

The results of suture are well known to differ widely in different nerves. A nerve like the musculo-spiral, which subserves simple and elementary functions, is often restored so perfectly that only the closest examination can detect any departure from the normal in the parts supplied. With nerves like the median and ulnar the case is very different. These nerves subserve delicate and complex movements, the execution of which requires not only the proper motor impulses but also the integrity of a complicated sensory mechanism. One has only to contrast the size of the nerves of different muscles in order to realize how much more is required of regeneration in the case of the more complex nerves. For example, in relation with the bulk of muscle supplied, the nerves to the glutei are ludicrously small when compared with the deep branch of the ulnar. We speak of these nerves as motor without always realizing how many of their fibres are afferent, and carry those sensory impulses which must necessarily reach the brain before the muscles can be thrown into orderly and co-ordinated action.

I have never seen a case in which restoration of function after suture of the median or the ulnar nerve could be said to approach anywhere near the normal, or to attain anything like the degree of perfection so often reached in the case of the musculo-spiral. The same point is illustrated in the ulnar and median nerves themselves. Many late results show how far in advance of the small muscles of the hand is the recovery of the muscles of the forearm. The accuracy and delicacy of movement required of the hand muscles are not demanded to anything like the same degree in the great flexors of the wrist and fingers. A recent examination of a number of patients from two to three years after suture of the median or the ulnar nerve has revealed the fact that the sense of position and the sense of passive movement in the fingers are almost wholly absent even in cases which, as regards voluntary movements, electrical reactions, and some other forms of sensibility, show good evidence of recovery. As an example I may briefly relate the following case:

Rather more than two years ago the patient was shot through the left arm, sustaining a large flesh wound and a comminuted fracture of the humerus. The brachial artery and median nerve were severed, and the ulnar nerve contused. Four months later the median nerve was sutured and the ulnar freed. At the present time the nutrition of the hand is fairly good, and the movements of all the joints except the metacarpo-phalangeal joint of the thumb are free. The ulnar nerve has almost completely recovered. The median muscles all react well to faradism, can be thrown into contraction voluntarily, and are of good bulk. The median area, however, is almost completely insensitive both to superficial and deep stimuli. Now whilst, with the aid of vision, this patient is able to bring the pad of the thumb against the pad of each finger in turn, not by trick movements but in a normal manner, and can hold an object between the finger and thumb fairly naturally, yet he can do none of these things with his eyes shut. Considering the nature of the concomitant injuries, and the fact that the main artery had been tied, this may be said to represent from the nutritional and motor point of view a fairly good recovery; nevertheless, owing to the profound sensory defect, the hand is of very little use.

In order that useful function may be regained it is necessary that sensibility, as well as muscular power, should be restored. This is specially true of the median nerve, and, unfortunately, the recovery of sensibility in the area exclusively supplied by this nerve is extremely slow, and rarely satisfactory. From the sensory standpoint the median is more important than the ulnar, whilst, as far as the hand is concerned, the ulnar is the more important motor nerve. Patients soon learn to disregard the inconvenience of an insensitive little finger, but an insensitive index finger is so incapacitating as to render the hand of very little use for most of the ordinary purposes of life.

Late functional results after nerve suture may be unsatisfactory from causes other than faulty regeneration. A patient with good muscles and supple joints, and with good recovery both of sensibility and of electrical reactions, may be quite incapable of making use of his limb. The importance of the psychological aspect of such cases is now too well recognized to need more than a passing notice here, and I only mention it in order to point out another disturbing factor in the estimation of results. The value of re-education can scarcely be overestimated.

In judging results by the crucial test of function, some standard of comparison must be employed. Normal hands vary so greatly in dexterity that it would be unfair to expect the same degree of excellence to be reached in all cases. Unconsciously, perhaps, we are apt to compare a hand under examination with our own, or with some ideal conception of the normal hand; nor do we always allow for the ordinary difference between right and left hands. Clearly the standard of recovery which must be set up for any individual should be based upon what he could do before the injury, and to this his previous occupation and the nimbleness of his other hand constitute a good guide. Two cases, recently re-examined, will suffice to illustrate this point.

In each the hand was in a good state of nutrition, and the joints and tendons were free from stiffness. Both patients had had the right ulnar nerve sutured some two years previously—in the one case just above the wrist and in the other just above the elbow. The former had made a good recovery so far as the hand muscles were concerned, whilst the sensory recovery was poor. The other patient had made an excellent recovery both from the motor and sensory standpoints. From the point of view of regeneration, therefore, the one represented a poor and the other a good result. Regarded from the functional point of view, however, the estimation of these two results had to be reversed. The man whose anatomical and physiological result was poor had large, clumsy hands, which seemed to differ very little from one another. With the hand of the injured limb he could do almost everything that he had been able to do before being wounded. He was a good fast bowler at cricket and now is as good as before; he can write as well as ever, and he can "do almost anything" with the hand. His wage-earning capacity as a labourer has scarcely, if at all, depreciated. The other patient, with a far better nerve recovery, is very differently affected. He is able to use the hand fairly well for all ordinary purposes, including writing, but not so well as before he was wounded. By profession a violinist, he is no longer able to earn his living with the violin, as he has lost his touch with the bow to such a degree that he has had to give up that instrument and has taken up the trombone. His value as a trombone player in the orchestra is twenty shillings a week less than it was as a violinist.

2. My second group of cases requiring operative treatment concerns those morbid conditions of an injured nerve which are evidenced by nutritional and vasomotor changes in its area of distribution, and by causalgia.

There can be little doubt that the great majority of what are sometimes loosely termed "trophic" changes are in reality the direct outcome either of neglect or of immobilization, whether imposed by splinting or by paralysis. Most of such changes can be prevented by the early application of appropriate measures, and many can be cured or at least relieved by properly directed physiotherapeutic treatment. They are apt to be encountered in any case of complete interruption of conductivity of a nerve. We must remember, too, that nutritional disturbances result from damage to the main artery of a limb.

Disordered vasomotor activity, however, is on a different footing. It is associated with partial and irritative lesions, and is of course seen at its worst in the area of distribution of those nerves which are particularly rich in vasomotor fibres—namely, the median, ulnar, and internal popliteal. The existence of disordered vasomotor activity naturally affects the end-result, especially if it has been present for any considerable period before operative treatment is undertaken, since profound nutritional changes will already have occurred not only in the joints but in all the other affected structures as well. Resection of the damaged portion of the nerve, with end-to-end suture, will at once put an end to the active trophic changes and allow recovery to commence under physiotherapeutic treatment. The process of regeneration will be prejudiced by the morbid changes which will have occurred in the nerve, and the stiffness of the joints will further impair the ultimate functional result. The end-results in these cases, therefore, can never reach the highest level.

Let us now turn to the end-results of those cases in

which disordered function is evidenced by that severe persistent and paroxysmal pain which is known as causalgia. It is well known that the pain, in the majority of these cases, tends very slowly to abate under suitable treatment, and that in the less severe cases no operative treatment is called for. On the other hand, it is unfortunately within the experience of all of us that there are certain cases for which no surgical treatment, even to the intraspinal division of all the posterior roots connected with the affected area, seems to be of any avail. Between these two classes, those spontaneously recoverable and those apparently incurable, there are a number of cases which operative treatment does benefit, and in which the ultimate results are excellent. The treatment of causalgia by operation is rather, I think, to be encouraged than otherwise. When successful it not only relieves suffering, but allows of the institution of physiotherapeutic measures which, by reason of the pain, have necessarily had to be omitted. Further, the "trophic" changes so often associated with causalgia may be arrested. These factors have a very great influence upon the ultimate recovery of function in the limb. From one point of view it is, perhaps, somewhat unfortunate that in many instances the causalgia is associated with so little paralysis or loss of sensibility in the area of distribution of the affected nerve. It is this fact which makes the surgeon hesitate before embarking upon an operation which, whilst it will probably relieve pain, will also interrupt still further the conductivity of the nerve. The injection of alcohol into the nerve well above the site of injury often affords immediate relief, but at the same time there is considerable danger of producing a lesion which may never be completely recovered from. The weaker the alcohol employed, the less will be the chance of producing so undesirable a result, but at the same time the pain cannot be so certainly relieved by the more dilute solutions. The best results are obtained when the strength of the alcohol is so adjusted as to relieve the pain sufficiently to allow of movement and massage being employed without seriously affecting either the motor or sensory functions.

RESULTS OF OPERATIONS FOR NERVE INJURY AT THE EDINBURGH WAR HOSPITAL.*

BY

MAUD F. FORRESTER-BROWN, M.S., M.D.Lond.

THE results of a nerve operation can be measured in two ways: (1) anatomically, (2) in terms of function, that is, working capacity.

These two by no means coincide; thus in certain cases the hand is very useful, even although the ulnar nerve is entirely unrecovered; in others the nerve may show recovery of every one of its functions as ordinarily tested, yet the grip may be weak, the co-ordination poor, and the patient may tell us that the hand becomes cold and useless in bad weather. Again, a sutured musculo-spiral may show persistence of the anaesthesia, yet if the muscles are recovered the hand will probably be capable alike of heavy and of delicate work; whereas after a median suture, if the thumb and index finger fail to regain the normal delicacy of touch in fullest measure, then the hand remains clumsy, and often altogether useless.

As regards anatomical recovery, it may be said at once that the cases in which it is complete within two years of the operation are in the minority. The writer has not had the opportunity of examining more than a few cases at longer intervals.

The following remarks are based on the analysis of a consecutive series of 643 cases, dating from 1916. Of these, 360 have been re-examined by the writer at periods of from six months to two years after their operations, some of them reporting at regular intervals. A few more cases have been included on whom detailed reports were received from the surgeons at other orthopaedic centres.

Nerve cases show a very variable degree of recovery of the different functions—motor, sensory and trophic—the variation not being constant for any particular nerve, time after operation, or level of lesion, as far as could be made out. In classifying results, therefore, it was considered that the only fair method was under separate headings for each function. The amount of return of each function also shows many gradations, which could not possibly be indicated in a comprehensive chart; therefore three grades only have been recognized: complete, incomplete, nil.

By "complete" motor recovery is here indicated return of voluntary power in all muscles supplied below the lesion, even where their strength is not up to normal, for the latter probably only depends on time and exercise, while the "normal" for any given individual is too difficult to estimate. "Complete" sensory return is used to include cases in which pressure, wool, and prick were felt and correctly localized over the whole distribution of the nerve while joint sense had returned. "Complete" trophic return implies the absence of muscle-wasting and of changes in the skin and its appendages. "Incomplete" includes all cases which, failing to fulfil the above conditions, yet show evidence of nerve regeneration.

In considering the results of nerve suture as measured by the patient's capacity for work, it is important to remember that some men are at work for which they do not need to use the injured limb. A series of cases of right-hand injuries, in which this error is unlikely to occur, was therefore studied, but the figures showed no great divergence on this account.

Out of 643 cases of nerve injury operated on in the Edinburgh War Hospital, 552 were able to be traced and 396 were personally examined.

The written reports from the 156 not examined were included only in the study of their capacity for work.

A. *Capacity for Work.*

Out of 237 cases of nerve suture, 12 per cent. are at ordinary work, 35 per cent. at light work.

The cases with upper limb injury show a slightly higher percentage at work than those with lower limb injury.

Even cases of musculo-spiral suture give the same percentage at ordinary work, but rise to 54 per cent. at light work.

A series of 95 cases of injury to the right upper limb show only 8 per cent. at ordinary work, but this factor does not affect the percentage at light work.

An analysis of the figures for the individual nerves shows 15 per cent. of ulnar sutures at heavy work—that is, above the average; whereas 56 per cent. medians are at light work, and in this above the average. This is interesting, as it confirms one's impression that, in regard to skilled capacity, anaesthesia of the thumb and index is more disabling than the general weakness produced by the loss of the ulnar intrinsic muscles. The contrary appears to be the case where rough use of the limb is required.

Out of 187 neurolysis cases, 8 per cent. are at ordinary work, 36 per cent. at light work. Thus they are slightly less satisfactory than the suture cases, the contrary of what might have been expected.

Out of 81 tendon transplants for various lesions of the upper limb, 23 per cent. are at ordinary work, 36 per cent. at light work. This is markedly better than the figures for nerve sutures.

B. *Degree of Recovery after Nerve Suture.*

Out of 158 suture cases, 28 per cent. showed complete motor return, 19 per cent. complete sensory, 21 per cent. complete trophic—50 per cent. incomplete of all three functions.

Total complete recovery (as defined above) was found in 5 median and 8 musculo-spiral cases. Almost complete recovery in several ulnar cases.

Absence of any recovery was found in 1 median and 6 transposed ulnars. These latter developed neuromata and were sutured again, after which some began to recover. The others could not be traced.

As regards individual nerves, the median (35 cases) showed 50 per cent. complete motor and 28 per cent. complete sensory recovery. The ulnar (53 cases) showed 17 per cent. complete motor and 13 per cent. complete sensory recovery, and 66 per cent. incomplete of both. The musculo-spiral (21 cases) showed 62 per cent. complete motor and 33 per cent. complete sensory recovery. Thus the apparently favourable nature of lesions of this nerve is due rather to the insignificance of its sensory distribution and to its relatively simple motor functions than to any speciality of the process of regeneration in it.

As regards nerve graft and flap operations, out of 14 cases, 1 showed complete motor return, 2 incomplete motor return, 4 showed sensory improvement.

Three nerve grafts were subsequently explored, and the grafts were found recognizable as nerve tissue, but they were embedded in dense scar, they remained of smaller calibre than the nerve into which they had been grafted, and they failed to respond to direct faradic stimulation.

Recovery of Neurolysis Cases.

Out of 117, 43 per cent. had full motor function and 19 per cent. full sensory. All except ½ per cent. showed some improvement, and no case was made worse by the operation. This suggests that exploration is advisable in all cases of peripheral nerve injury when steady improvement does not occur

* This communication forms part of a thesis approved for the degree of M.S. of the University of London.

spontaneously within one to two months of the healing of the wound.

It might have been thought that the figures for neurolysis would be better than those for nerve suture; but unfortunately this is not the case, for they often represent lesions with destruction of a certain number of nerve fibres, which are not restored by neurolysis, while the importance of the fibres which are intact contraindicates resection and suture. Even a detailed study of these results is inconclusive, because they involve so many variable factors, such as the nature of the lesion (compression, presence of a neuroma, degree of invasion of the sheath or nerve by scar tissue, etc.); also the judgement of the surgeon as to the suitability of the operation; also the degree of loss of function before operation, which, unfortunately, has often been only vaguely recorded, whereas the strictest accuracy alone could enable us to judge the result.

Results of Tendon Transplants.

Of 96 transplants (both upper and lower limbs) all except one gave useful restoration of the lost function, while many were excellent.

The transplants for musculo-spiral paralysis, where some nerve regeneration had later occurred, produced practically normal hands, and many were at heavy work. This suggests that there is no need to fear subsequent upsetting of the muscle balance if regeneration occurs, and therefore in cases of severe incomplete lesion of the musculo-spiral, and others where the prognosis after suture is poor, a transplant should be done without hesitation.

The functional results after transplantation were also noted to be better than those after suture; but it must be remembered that whereas a transplant is fit for light work in two to three months after operation, a suture has not the same strength till about ten months in the most favourable case, so that the working capacity of suture cases may be expected to go on improving for two years or more, while the transplant has attained its maximum in much less time. This advantage of the transplant operation is not always remembered by surgeons.

As regards order of recovery of the various functions, there is also a good deal of variation, and, unfortunately, the incompleteness of our records prevents us from giving comprehensive figures. In the cases of which frequent progress notes could be kept the following points were observed:

Tinel's sign of tingling referred to the distribution of the nerve, and elicited by tapping it at a point corresponding to the level reached by the down-growing axones, was usually the earliest sign, and was obtained about three to four weeks after operation at a point slightly below the suture level, from which it descended more or less regularly in correspondence with the return of function at the various levels. It is noteworthy that this sign has been absent in a few cases which recovered, and that it has been present in some which have so far shown no other evidence of recovery. The next sign to appear was usually tenderness to pressure of the muscles supplied immediately below the lesion.

Progress after this depended on the level of the lesion. If muscles were supplied close below it, while the skin distribution was remote, then motor return preceded sensory. It must be remembered that small degrees of sensory improvement are easier to determine than faint voluntary contractions in a muscle. Thus lesions of the median above the elbow usually gave recovery of the flexor carpi radialis as their first sign, the pronator radii teres being difficult to determine; whereas lesions at the wrist showed recovery of joint sense and deep pressure sense before any appreciable contraction of the intrinsic muscles could be made out. Epicritic sensation (to wool and prick) returned in the palm and sole sometimes before deep sensation was completely returned in the digits, the anaesthesia receding peripherally, like the ebbing tide.

With regard to electrical reactions, a faradic response sometimes returned in the muscles before voluntary power, but usually the converse was the case. In cases of deficient re-education, especially with the interossei muscles which are difficult to train, the faradic response came first.

The figures which the writer was able to work out proved interesting in a negative rather than a positive sense. For instance, they showed that, within the limits of such a series, there was no constancy in the rate of recovery of any particular nerve; that the rate was not definitely influenced by the level of the lesion, nor absolutely by the period since operation. Tables were also worked out which showed that, in this series, the degree and rate of recovery were not influenced by:

1. The length of time between the injury and operation.
2. The duration of the initial sepsis.
3. Recrudescence of sepsis after the nerve operation, which occurred in a certain number.

The observations recorded above suggest that every case of nerve lesion which has not recovered spontaneously within a reasonable time should be given a chance by operation, since regeneration of nerve fibres after suture can be looked for with confidence in the majority of cases, although progress must be expected to be slow. Whether recovery will ultimately be complete in the majority of cases it is too early yet to say, and the prognosis of any individual case is very hard to estimate.

LATE RESULTS OF OPERATION.

BY

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I PROPOSE to confine my remarks to the late results of operations designed to bridge a gap in a nerve which cannot be repaired by end-to-end union, because about these will be found the greatest divergence of opinion, and because I am convinced that in this field any future great advances in the surgery of injured nerves will be found.

I think that sufficient emphasis has not been laid on the fact that operation is merely an episode in the treatment of a divided nerve. The most that any operation on a divided nerve can do—and this is the aim of operation—is to place the ends in the position most favourable for nature to accomplish the downgrowth of axons from the proximal into the distal segment. It is most difficult in these cases to analyse an end-result and to estimate how much of it is due to the efficiency or the inefficiency of the operation, and how much to the efficiency or inefficiency of the pre-operative and post-operative treatment by physiotherapeutic methods.

It should be remembered that the war-time cases in which a gap in a nerve cannot be repaired by an end-to-end union are, because of the severity of the wounds, just those in which pre-operative treatment by physiotherapeutic methods is likely to have been inefficient and in which the post-operative treatment is beset with difficulties.

In a personal series of 161 operations on nerves there were 22 cases which at the time I considered to be irreparable by end-to-end union. I know that some of the earlier ones I could now have treated by end-to-end suture.

These were dealt with as follows:

6 musculo-spiral injuries by alternative tendon transplantations;

6 ulnar nerve injuries were repaired by double lateral implantation into the median;

8 cases were dealt with by free autogenous transplants:
- 1 great sciatic,
- 1 posterior tibial,
- 3 ulnar,
- 3 median.

2 cases were dealt with by homologous transplants:
- 1 musculo-spiral,
- 1 ulnar.

Tendon transplantation for irreparable injuries of the musculo-spiral nerve gave results which left nothing to be desired, and, in my opinion, is the operation of choice.

As an indication of the bad conditions which are likely to be present in cases requiring nerve transplantations or operations of a similar nature in war-injured patients, the following facts may be noted:

In the 6 cases of double lateral implantation:

Average time between date of wound and healing of wound	99 days.
Average time between date of wound and operation	313 ,,

In the 8 cases of autogenous transplantations:

Average time between date of wound and healing of wound	188 days.
The longest time being	539 ,,
Average time between date of wound and operation	426 ,,
The longest time being	1,088 ,,

All had wounds which had suppurated severely, and all except two were associated with severe injury to bone. Extensive scarring, chronic myositis, fibrosis, and stiff joints were met with in every case.

If degeneration of nerve is, as I believe it to be, as much a vital process as regeneration, and since the best conducting path for regenerating axis-cylinders is a piece of degenerated nerve, then a strong theoretical argument against free autogenous transplantation is the fact that placed in the gap is a piece of nerve which has been separated from its blood supply and is therefore dead.

This objection is not applicable to the operation of so-called double lateral implantation. The expression "double lateral implantation" of an injured into an intact nerve has created a confusion of thought, and is partly responsible for the objections which have been raised to

the operation. I have already pointed out that the so-called operation of double lateral implantation of an injured into an intact nerve is really that of a pedicled autogenous transplant of the intact into the injured nerve. Thus, a double lateral implantation of the ulnar nerve into the median should really be called a pedicled autotransplant of the median into the ulnar.

Of the six cases of so-called double lateral implantation in my series, two showed recovery of protopathic sensation over the ulnar area and faradic response in the ulnar intrinsics, without voluntary action, at the end of twenty-four months. Two cases were lost sight of at the end of five months, and two showed doubtful signs of recovery at the end of nine months.

Sir Charles Ballance has kindly allowed me to mention a case I have had under my care for two years—one of division of the median nerve in the forearm, operated upon by him in Malta in 1916. He made a double lateral implantation of the median into the ulnar, or, as I should prefer to call it, a pedicled autotransplant of the ulnar into the median. When I first saw the man he had complete wasting and paralysis of his thenar eminence, with R.D. in the opponens and abductor brevis pollicis. The opponens has now recovered with good voluntary action and faradic response, and the sensation of the hand is normal except over the proximal phalanges of the index finger and thumb. He has still wasting and loss of voluntary action in the abductor brevis pollicis. The patient, at Sir Charles Ballance's request, has come to the meeting to show himself.

In view of the severity of the injuries and of the associated lesions, the results of pedicled autotransplants of the median into the ulnar, in my experience, do not compare unfavourably with those of end-to-end union of the ulnar in the forearm. In no case has any permanent damage resulted to the median nerve, and two show as good results as the majority of end-to-end unions of the ulnar in the forearm—namely, recovery of sensation and faradic response in the intrinsics without recovery of voluntary action.

The results of free autogenous transplantations have been less favourable.

One ulnar case in the upper arm showed voluntary power and faradic response in the flexor carpi ulnaris nine months after operation, but no sign of recovery in the ulnar intrinsics.

One in the forearm showed faradic response in the adductor pollicis and first and second palmar interossei eighteen months after operation without voluntary action.

Another in the forearm showed no sign of recovery fifty-four months after operation.

One of the sciatic in the middle of the thigh showed recovery of good voluntary action and faradic response in the calf muscles of the leg thirty months after operation, with incomplete recovery of sensation and no recovery in the anterior tibial or peroneal muscles.

The three median cases showed no sign of recovery at the end of five, eight, and nine months respectively.

Both Sir William Thorburn and Mr. Sargent have in their opening remarks condemned the operation of so-called double lateral implantation, and Mr. Sargent has said that the operation of nerve transplantation has no practical value. Sir Robert Jones and Sir Berkeley Moynihan have made statements to the same effect.

When one remembers the delicacy of an axon, our relative ignorance of their minute anatomical distribution, and the difficulties which must beset their transport across a gap in a nerve, the fact that the results I have laid before you are not good is not surprising. Rather is it to be wondered at that any recovery takes place at all; but that such does occur encourages me in the belief that the operations of pedicled autogenous transplants and of free autogenous transplants are deserving of further study and research, and still have a place in the operative surgery of injured nerves.

DISCUSSION.

Mr. J. A. COPLAND (Leeds) mentioned three cases of median nerve grafting which appeared to be successful. He strongly advised avoidance of traction and the utmost gentleness in manipulation. Foreign membranes should not be used. A new bed of perfectly healthy tissue should be made for the nerve by the displacement of muscle or fascia flap. He suggested another view regarding "trick" movements: their object was ultimately a good economic result. If patients developed a capacity for replacing paralysed muscles, it was the surgeon's duty to encourage them to use the muscles in such a way as to make them functionally useful. The capacity for active muscular power returning to replace a damaged median should be encouraged. It meant that an improved vascular supply was the consequence. He had observed a median case in which nine months after suture in the arm recovery of every muscle in the median supply occurred; it was the most rapid in his experience. It was important to place the nerves in proper axial rotation. In many cases it was possible to see with the naked eye the arrangement of bundles, so that the appropriate bundles could be brought into apposition with one another. A very important matter in after-treatment was the maintenance of warmth of the limb; it hastened recovery.

Mr. ROWLEY BRISTOW said that in the Military Orthopaedic Hospital, Shepherd's Bush, the results agreed in the main with those quoted by the leaders in the discussion. In examining nerve suture cases he had observed that those nerves which early showed some sign of regeneration did not of necessity go on to better ultimate function than others, and he did not put any special value on early signs of recovery. His experience of grafts was that all were failures; there were six in number; of these, one only might be said to be a moderately satisfactory result. Some details of this case were narrated; the patient did not continue to make further recovery in two years, and it was probable that the apparent good result was due to two of the muscles having obtained new nerve connexion. The best he could say of grafting was that some isolated muscle gave evidence of returning function after such an operation. Tendon transplantation was advocated by him in cases of musculo-spiral injury which had recovered imperfectly. Ulnar cases, as a rule, gave a very poor muscular recovery; there was only one way to improve muscles in the ulnar supply, and that was by electrical stimulation of each individual muscle, and not by general electrical treatment of the whole group of muscles in the hand.

Mr. NAUGHTON DUNN (Birmingham) pointed out that nerves should not be too closely sutured. In two cases sutured under great tension he obtained very rapid recoveries. His results had improved since he began to leave a very little gap between the ends of the nerve. In two cases of nerve grafting no sign of regeneration in the graft could be found when operated on again nine or ten months afterwards. He thought it was unfortunate that cases came from France with primary suture performed, for many of them were kept waiting in the belief that the sutures would hold and that further operation would not be necessary.

Dr. L. E. SHORE (Cambridge) said that, after listening to the addresses of the leaders in the discussion, it was not possible to give a complete answer to many of the points raised. As a physiologist, he had to admit that the knowledge which the surgeon wanted the physiologist could not give. He would remind them that when peripheral nerves were very finely teased out it was found that the axis-cylinder divided into a large bunch of minute axis-cylinders, and these little ones seemed to hunt for the tracks downwards; successful bunches did find the path; many others failed and simply groped about in the connective tissue. To attempt an absolute anatomical end-to-end suture was perfectly hopeless. Nerve fibres in the nerve, as it was dissected down, constantly changed their position from one side of the nerve to the other, running into various groups all the time. That did not alter the fact that each particular axis-cylinder had its own proper place. As to physiological recovery, it was an important point when the patient could begin to move the group of paralysed muscles in a mass—that was to say, the whole group acting—but the patient could not till a much later stage pick out the individual muscles. It had recently been suggested that, in thinking of re-education, two classes of motor nerves had to be dealt with: one to control the whole mass of muscles as a group, and the other controlling finer muscular movements, picking out individual muscles, though he was not sure that there were sufficient grounds to support that view. In regard

to economic recovery, that depended very much upon recovery of the degree of sensation, and until the patient had epicritic sensation fully recovered his limb was not of full economic use. The musculo-spiral seemed to recover much more quickly than other nerves, but he was critical of that idea, and believed that its apparent economic recovery was due to the fact that the movements of muscles supplied by the musculo-spiral were relatively unimportant, and that the reason why the median recovered so slowly was that that nerve was concerned with very fine movements. Although it was true, in regard to the ulnar nerve, that there was slow recovery of the intrinsic muscles of the hand, the patient's hand was not impaired so much as in a median defect, because the little finger was not so important as the index finger, say, for picking up objects and other similar fine movements. It was important, in regard to the economic question, to note the nature of the man's hand and his work, and what he was expected to do with it. Dr. Shore suggested that if a big gap in a nerve existed, could not the bone be shortened so as to bring the nerve end-to-end rather than resort to grafting? He narrated a case which he observed at operation, in which the electrical test produced movements of the muscles in the ulnar distribution long before there was any sensory recovery. In this instance, the nerve which was being tested was exceedingly hard and board-like, so that while it seemed able to transmit impulses resulting in muscular movements, sensory impulses were not allowed to pass.

Mr. HARRY LITTLEWOOD, Vice-President of the Section, spoke of the work on the surgery of the nerves which had been carried on at the 2nd Northern General Hospital; some of it had been already recorded in the *Medical Record*, 1919. With regard to the observations requiring to be made on a patient after operation, he thought that outside observers should be asked to make these observations and give opinions on the work of the surgeon, because the surgeon could not himself give an unbiassed opinion on his own work. There might be some kind of accountant watching the work of the surgeon. If this were done, there would be fewer errors in observation. They knew by experience that some results of the union of divided nerves were sufficiently encouraging to enable them to go on operating, and patients themselves admitted that they were, on the whole, better off than without operation. Not enough stress was laid on improvement in function; very soon after the nerves were brought end to end there was evident improvement in function which amounted really to improvement in the vascular supply. In the treatment of causalgia he strongly advocated early operative procedures. In regard to the question of nerve grafting, he thought that the arguments of Mr. Joyce in favour of lateral transplantation were very good; even if only one or two cases in which nerve grafting was employed could be shown to have benefited, the operation could only be regarded as of distinct value. He was very glad that they had Professor Shore present at the meeting, and he hoped in the future for a closer association of the physiologists and the surgeons and also of the physicians and the surgeons. He pointed out that Dr. Shore's suggestion as to getting a portion of bone out so as to shorten the limb and enable the ends of a divided nerve to be brought together had already been carried out.

Openers' Replies.

Sir WILLIAM THORBURN, in replying to the discussion, said that shortening of the bone was particularly applicable in the upper arm, and, in his judgement, it was a method of operating more suitable for attaining the object of bringing the nerve ends together than the introduction of a nerve graft. The whole question of nerve suture was related to "shunting." There was no difficulty in overcoming the gross anatomical question of suture, but they had to do more than simple suture; they had to endeavour to help the axis-cylinders to find the right course. For axis-cylinders to find their course through the graft was a more precarious matter than for them to find their way into a distal portion of the nerve. He thoroughly approved of the appointment of some impartial tribunal to investigate these cases, and pointed out that one had actually been in existence in London for the past eighteen months or more. One result of investigation by this tribunal was, that it found itself opposed to the alleged success of cases treated by grafting. On the other hand, some of his own friends in whom he believed, did themselves believe that they had obtained successful results from nerve grafting.

Mr. PERCY SARGENT (London) remarked that a man examining his own cases could not help being prejudiced in favour of seeing good results. He wished to point out, in regard to the question of nerve grafting, that those cases which had been grafted, and which had apparently shown some sign of returning function, had usually been examined with greater minuteness after the operation than before; in some cases examination before the operation really showed that axons had been getting through, and these probably accounted for some of the successes in nerve grafting. It had been proved experimentally that the longest distance through which axons could grow was 1 cm. to 2 cm., and he thought it was impossible, therefore, for axons to grow through 2 to 3 inches of nerve graft.

SECTION OF NEUROLOGY AND PSYCHIATRY.

HENRY HEAD, M.D., F.R.S., President.

DISCUSSION ON DEMENTIA PRAECOX AND ITS RELATION TO OTHER DISORDERS.

OPENING PAPER

BY

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DEMENTIA PRAECOX constitutes without doubt one of the most important problems in public health with which modern medicine is confronted. It is a disease so widespread, so prolonged in its course, and so devastating in its effects, that it is responsible for a considerable majority of the cases to be found in the chronic wards of our asylums. Nevertheless, our knowledge of this disease is still wofully inadequate, and its nature, causation, and even its existence as a definite entity, are still subjects of constant dispute between different schools of thought. The only point upon which general agreement can be found is, unfortunately, the absolute impotence of any method of treatment. This impotence is clearly dependent upon the lack of any certain knowledge concerning the nature and causation of dementia praecox, and were these latter problems solved we might reasonably hope that much could be done for prophylaxis, even if curative treatment should still be impossible.

The preliminary problems, to which an answer must be found before the questions of prevention and therapeutics can be satisfactorily approached, may be said to be three in number: first, To what extent is dementia praecox a definite entity? secondly, What is its nature and causation? and thirdly, What are its relations to other disorders? These are the questions to which, as I conceive it, attention should be mainly directed in the discussion which is to follow. The purpose of this introductory paper will be, not to furnish an answer to these questions, but to indicate the points at issue and the views which are held by various schools of thought regarding them.

To what Extent does Dementia Praecox Constitute a Definite Entity?

Although, prior to the advent of Kraepelin, a number of clinical pictures met with mainly in adolescence and early adult life had been separated out from the mass of the psychoses, notably hebephrenia, katatonia, and Clouston's "insanity of adolescence," it is to Kraepelin that we owe the first clear formulation of dementia praecox as a definite disease.

He was led to this conception in pursuance of his attempt to classify mental disorders, not merely according to the prominent symptoms which they might display, but into groups characterized by a definite course and outcome—a line of investigation which has marked one of the turning-points of modern psychiatry. He observed, as Clouston had done, that a

number of adolescent cases tended to progress to a well marked terminal state of mental enfeeblement, but he noted further that, although the clinical pictures presented by these cases in their earlier stages were in many respects very different from one another, yet it was possible to discern certain common features whose recognition would enable one to predict that an ultimate dementia was likely to occur. These features were chiefly a "weakening of the emotional activities which permanently form the mainsprings of volition," and "loss of the inner unity of the activities of intellect, emotion, and volition, in themselves and among one another." He therefore concluded that the clinical pictures thus characterized, however different they might be in other respects, were due to the same essential morbid process, and that it was justifiable to regard them as manifestations of a single definite disease, to which he gave the name of dementia praecox.

Since Kraepelin's conception was first formulated there has been much controversy as to its validity and value. For some time it was disputed whether any such entity could be separated out from the general mass of the psychoses, and although it may now be said that this question has been definitely answered in the affirmative, there is still the greatest uncertainty and divergence of opinion with regard to the delimitation and relationships of the disorder. This is clearly inevitable so long as the essential morbid process, whose existence Kraepelin assumes, remains undetermined. Numerous hypotheses, both pathological and psycho-pathological, have been put forward as to the nature of this essential process, but no one of them has yet received general acceptance. The psycho-pathological theory proposed by the Freudian school is precise and clear-cut, and for those who adhere to this point of view dementia praecox is a very definite and well marked entity, although its boundaries are probably not altogether identical with those suggested by Kraepelin. Unless this theory is accepted, however, the problem of the basic change in dementia praecox must be held to be still unsolved, for no really comprehensive and satisfying theory has yet been put forward from the pathological side. For the present, then, dementia praecox must stand or fall as a clinical and not as a pathological entity.

From this standpoint it must at least be admitted that there are a vast number of cases which present the characters and course defined by Kraepelin, and that they constitute an easily recognized clinical group. It seems impossible, however, to set any clear-cut boundaries to this group. All around the central kernel of typical cases we find cases shading gradually off into disease pictures whose relationships are uncertain, and which link up with other disorders of an altogether different character. Kraepelin's own views concerning the boundaries of dementia praecox have changed considerably, and the conclusions he has reached are, in the last edition of his textbook,[1] very cautiously and tentatively expressed. He frankly admits that it is quite possible that the borders of the disease are drawn at present in many directions too narrow, in others too wide. He has himself considerably reduced the area of dementia praecox by separating from it a group of cases which he formerly included in dementia paranoides, and to which he has now given the name of paraphrenia.

We may conclude, then, that dementia praecox has established its claim to be regarded as a clinical entity in the sense that it marks off a group of cases with notable similarities in their symptomatology, course, and outcome, although the borders of this group cannot be accurately defined. To what extent it can claim to be an entity in a more exact sense must depend upon the success with which a specific morbid process can be shown to underlie it. We may pass on, therefore, to consider the various hypotheses which have been put forward with regard to this latter point.

What is the Nature of the Essential Morbid Process in Dementia Praecox?

The views which have been expressed in answer to this all-important question fall into three groups: (1) Physiogenic theories, which maintain that dementia praecox is due to changes occurring in the bodily organism, and that the psychical phenomena are purely secondary and without causal significance; (2) psychogenic theories, which maintain that the essential causes are psychical in order, and that the physical changes are secondary; (3) theories which attempt to combine both these view-points.

The physiogenic theories are based on the indubitable fact that changes in the brain and other organs have been demonstrated to occur in dementia praecox, although there is no general agreement as to their precise nature and significance. So far as the histology of the brain is concerned, various phenomena have been described by different observers, but the changes almost universally found in cases of any duration consist in degenerations of the cortical cells, with glia proliferation. Mott[2] maintains that similar degenerative changes can be demonstrated in the basal ganglia and pons. It can hardly be claimed, however, that these findings are specific, because similar phenomena can be seen in other mental and organic brain diseases. Nevertheless, they clearly constitute a formidable obstacle to the acceptance of any purely psychogenic theory of dementia praecox.

Changes in other organs of the body have been described by various investigators, but in most cases there has been no strong ground for believing them to be other than accidental. Mott,[2] however, in a recent paper brings forward some observations which may prove to be of the greatest importance:

> In an examination of the testes and vesiculae seminales of twenty-two cases of dementia praecox he found evidence of marked atrophic changes, ranging from an alteration of the biochemical reaction of the heads of the spermatozoa to a complete regressive atrophy of all the seminiferous tubules, so that "the appearance of the organ as regards capacity for function in an adolescent was less than that observed in the decrepit dement of 86. Indeed, the testis of an old dement of 80 showed more microscopic evidences of virility than any one, even of the earliest cases, of dementia praecox." Similar retrogressive changes have been found in the ovaries of female patients. Mott considers that the atrophic phenomena in the reproductive organs are an expression of the same cause, premature senility or biochemical deficiency, as is responsible for the cerebral changes, and that "dementia praecox is dependent upon a progressive degeneration of the nucleus and the nucleoprotein basophil substances of the neurones, unattended by vascular or perivascular or meningeal changes, and dependent upon or associated with a chemical alteration of the nuclear substance."

The view that dementia praecox is to be ascribed to a degeneration of the cerebral neurones due to an initial insufficiency or instability arising from hereditary factors is widely held. Other hypotheses that have been proposed are that it is the result of a bacterial toxin, that it is an autointoxication, and that it is a disorder of the internal secretions of the sexual glands. The evidence produced in support of these various suggestions is not, however, of a very convincing character.

The first noteworthy attempt to interpret the phenomena of dementia praecox on the basis of a psychogenic theory was made by Freud[3] in 1896.

> In a paper published in that year he attempted to analyse a case which he described as one of paranoia, but which from internal evidence was clearly an example of paranoid dementia praecox. He showed that the symptoms could be regarded as the result of psychological mechanisms similar in kind to those he had found to exist in hysteria and the compulsion neurosis.

This thesis was developed further by Jung[5] in his work on the psychology of dementia praecox published in 1907.

> He confirmed Freud's view that the symptoms were due to the existence of repressed systems, to which he gave the name of complexes, and held that the patient's whole mental life became more and more concentrated in these complexes, with a consequent withdrawal from reality and loss of adaptation to the environment. At that time, however, he considered it an open question whether the complexes were actually the cause of the disorder, or whether the cause really consisted in some physical agent which produced a fixation and morbid growth in complexes already existing.

In his later views Jung[6] has diverged materially from Freud's standpoint.

> He maintains that the disturbances which occur in dementia praecox cannot be described in terms of sexual libido, and has developed in place of the latter the conception of a general psychic energy of which the sexual libido is only one mode of expression. Dementia praecox arises owing to a failure of adaptation to the actual environment and circumstances, and a consequent regression of the psychic energy to more primitive modes of adaptation. These more primitive modes are not merely infantile, but archaic, and resemble those which presumably occurred in the childhood of the race. The patient, in other words, finds it impossible to adapt himself to the actual world in which he has to live, and retires into an artificial world of his own built up of phantasies, in the construction of which archaic modes of thought play an important part.

Freud's conception of the development of the libido has undergone a noteworthy amplification in recent years,

largely as the result of investigations into the psychology of dementia praecox.

This consists in the interpolation, prior to the appearance of "object-libido" (the attachment of the libido to other persons, commencing with the parents), of an earlier stage in which the libido is directed towards the individual's own personality. This earlier stage is termed the "narcisstic" stage, and is held by Freud to constitute a phase in the growth of every normal child. The "object-libido" is then developed at the expense of a portion of this narcisstic libido. In dementia praecox there is a persistence of, or a regression to, the narcisstic stage of development, the libido being withdrawn from its external object attachments and directed towards the patient's own personality.

In this way Freud explains, on the one hand, the apathy and withdrawal of interest from the external world characteristic of dementia praecox, and on the other hand, the grandiose delusional symptoms common in this condition. As an interesting further development of this conception, Freud regards many of the symptoms of dementia praecox as secondary and distorted attempts on the part of the mind to re-establish points of contact with the external world—that is to say, they are not immediately connected with the essential morbid process but are, as it were, misdirected efforts at self-cure.[4]

The theories which attempt to combine both physiogenic and psychogenic points of view fall into two groups. The first regard dementia praecox as a disease of essentially physical causation, dependent upon factors in the physical organism, whether these be degenerations, developmental anomalies, or intoxications. It is admitted that psychogenic factors play a part, but this is held to consist merely in the colouring of the symptoms in particular tints—that is to say, the disease process which determines the development and outcome of the disorder is the result of purely physical factors; but the particular delusions and other symptoms displayed are fashioned by psychogenic factors, possibly complexes, conflicts, and so forth, acting according to the mechanisms described by Freud. This theory derives some support from the analogy with general paralysis—a disease whose essential causation is clearly physical, but in which the particular mental symptoms displayed can often be traced to psychogenic factors.

In the second group of combined theories an attempt is made to take account of both physical and mental factors as actual causal agents, and we may perhaps hope that in a theory of this character a really comprehensive and satisfactory explanation of dementia praecox will ultimately be found. It is clearly necessary that due weight be attached to both the pathological and psychological facts, and unless this is done a one-sided and inadequate view must inevitably result. On the one hand, the indubitable pathological findings in cases of any standing, the incidence of heredity (which Rüdin claims to have a Mendelian distribution), the progressive and deteriorating course of the disease, so unlike the course observed in disorders known to be predominantly psychogenic in origin, all make it extremely difficult to accept any purely psychogenic theory of causation. On the other hand, it is equally clear that the psychological theories put forward by Freud and Jung, however much these may differ from one another in certain details, provide a satisfactory basis into which the facts of clinical observation can be accurately fitted, and offer an explanation of the disease more comprehensive and illuminating than any of the physiogenic theories yet proposed. It would seem, therefore, that any adequate formulation of dementia praecox must combine both the physiogenic and psychogenic points of view; that the strife as to an "either—or" must necessarily be sterile, and that the final solution will be found in a "both—and." The direction in which this solution may be sought will perhaps prove to be in the conception of a biological reaction, the adaptation or attempted adaptation of an individual with a certain make-up to the environment in which he has to live, and which is capable of being expressed in both physiological and psychological terms. Clearly such a conception is in accord with the psychological theories which have been proposed, and it is not impossible to believe that the psychological reactions are the obverse of physiological reactions in which the pathological facts so far observed will be found to fall easily into their place. Freud's theory of the developments of the libido may obviously prove to be capable of expression in chemical and physiological terms, and we may perhaps venture to hope that endocrinology will ultimately provide facts which will enable a precise statement of this correlation to be made.

In the whole sphere of the psychoses and psychoneuroses the conception of biological reaction is tending to replace the old search for disease entities. It is questionable whether in mental disorders any such disease entities do or can exist, and it is more profitable to regard the patient as a psycho-physical organism reacting in one way or another to its environment according to the presence of certain factors. The "disorders" of psychiatry would then be names indicating these various types of reaction, and it would no longer occasion surprise or disappointment when it is found that these "disorders" shade one into another, and that no sharp line of demarcation can be made between them. In such a conception dementia praecox would take its place as a type of reaction to which many patients conform precisely and completely, and many others, who are not all to be found in asylums, to a lesser degree.

Relation of Dementia Praecox to other Disorders.

The view taken of the relation of dementia praecox to other disorders must obviously be largely dependent upon the position adopted with regard to the vexed questions discussed in the last section. The acceptance of a definite theory of causation would not only define the limits of the disease, but would also constitute the only basis on which relationships to other disorders could be satisfactorily determined. From the clinical standpoint, however, it is clear that there are a number of conditions which are either akin to dementia praecox in the similarity of the morbid processes involved and the clinical pictures displayed, or which are connected to dementia praecox by a chain of cases showing a gradual transition from one to the other. The most important of these conditions are mental deficiency, including imbecility and idiocy, the psychoneuroses, including hysteria and the compulsion neurosis, manic-depressive insanity, Meynent's amentia (exhaustion psychoses, confusional psychoses), paranoia, and Kraepelin's recently constructed conception of paraphrenia. A few words only will be devoted to each of these in this introductory address, in the hope of indicating points which may be taken up in the subsequent discussion.

Mental deficiency, including imbecility and idiocy, is related to dementia praecox in that cases are not uncommonly observed in which an initial state of weak-mindedness subsequently undergoes a degenerative process and passes over into a typical condition of dementia praecox. Kraepelin terms these cases "engrafted hebephrenia," and they lend some support to the view that dementia praecox is a degenerative condition due to an initial inadequacy and instability of the nervous and other systems. Kraepelin has also put forward the hypothesis that certain cases, apparently of idiocy, in which mannerisms and stereotypies occur are in reality instances of dementia praecox appearing very early in life. Whatever relationship may exist, however, it is clear that the two disorders are radically distinguished, in that mental deficiency is essentially a stable condition, whereas dementia praecox is essentially a progressive or rather regressive process.

The kinship of dementia praecox to the psychoneuroses is a subject of great interest and importance. Clinically many symptoms occur which are common to both groups, notably those which are found both in hysteria and in katatonia. The psychological investigations of Freud and Jung have shown that not only does this clinical similarity exist, but that the psychological mechanisms involved are closely allied in the two cases.

The difference for Freud lies in the fact that the psychoneuroses are "transference-neuroses" dependent upon disturbances in the fixation of the object-libido, whereas in dementia praecox there is a loss of object-libido due to the development of a narcisstic reaction. The same essential difference is regarded by Jung as a tendency towards excessive introversion in place of the extroversion characterizing the hysteric. The clinical fact upon which this distinction is based is, of course, universally admitted, and is described by Kraepelin as a lowering of the susceptibility of the emotional life in contrast to the heightened susceptibility of hysteria.

Jones [7] has developed the view that in both the psychoses and psychoneuroses we have to deal with various types of reaction to a fundamentally allied group of difficulties—namely,

intrapsychical conflicts of a biological nature—and that in both an introversion or turning away of interest from the external world is the most characteristic feature. He suggests that the distinction between the two lies in the degree to which this introversion takes place; that hysteria and dementia praecox "represent merely different stages and varieties of a profound introversion rather than two entirely distinct disease processes." He thinks it possible, moreover, that manic-depressive insanity and certain of the epilepsies may be regarded as representing intermediary stages in this introversion. Jones's paper on this subject was written in 1913, however, and it is not unlikely that he would now modify his views in the light of Freud's recent work on the significance of narcissism.

It is difficult to discuss the relation of dementia praecox to Janet's psychasthenia,[8] because the existence and delimitation of psychasthenia has been much disputed, and many of the cases which Janet described are regarded by the Freud school as cases of dementia praecox. This mere fact, however, is an interesting demonstration of the kinship which must exist between the psychoneuroses and dementia praecox.

The relationship between dementia praecox and manic-depressive insanity is a much disputed problem. When Kraepelin first developed the conception of dementia praecox the essence of his action was to subdivide Clouston's adolescent insanity into dementia praecox and manic-depressive insanity. His views concerning the delimitation between these two entities have varied considerably, however, and he has tended latterly to enlarge the manic-depressive group at the expense of the dementia praecox group. Clinically, although the typical examples of the two disorders are clearly very different, there are a large number of cases presenting features common to both in which a diagnosis is practically impossible. Considerable doubt has been expressed as to whether Kraepelin's distinction is really fundamental. Jones's view that manic-depressive insanity may perhaps be regarded merely as differing in degree from dementia praecox has been mentioned above.

Similar difficulties exist in the case of amentia (confusional insanity). Kraepelin considers that many of the cases ascribed to this group are in reality examples of dementia praecox, and that one can only properly differentiate the group of cases due to the toxins of infectious disease.

The distinction between paranoia and dementia praecox is another question upon which very divergent opinions have been expressed, but the tendency now is to regard the two conditions as differing in degree rather than in kind.

Kraepelin has recently separated off from dementia praecox a number of cases which he formerly regarded as paranoid forms of that disease, and has given to them the name of paraphrenia. The main distinguishing feature of this group is the slight development of the disorders of emotion and volition, in conjunction with marked intellectual disturbances.

It is clear that the delimitation of paraphrenia on the one hand from paranoia, and on the other from dementia praecox, must necessarily be a somewhat arbitrary matter, and Kraepelin's new conception would seem really to provide further evidence of an essential unity in all three conditions.

If the hypothesis proposed in the preceding section, that dementia praecox and those other disorders are types of biological reaction rather than definite pathological entities, should ultimately be established, then the problem of the relationship existing between them will clearly be regarded from a different angle. Just as the old attempts to divide mental disorders into diseases based on the clinical pictures presented were replaced by Kraepelin's division into diseases distinguished by their course and development, so will this again be replaced by the determination of the main reactions displayed by the individual case, and of the factors upon which these reactions depend. We shall no longer be disturbed by the fact that cases will not fit into pigeon-holes, because we shall expect to find mixed reactions. Dementia praecox and other names will merely indicate the predominance in a case of a particular type of reaction.

The psychological schools have already moved far in this direction, and the need of the moment would seem to be a correlation of the facts elicited by these schools with the findings of the anatomist and the chemist, in the hope that from this correlation there will ultimately be evolved a formula which will take due account of both the psychological and physical material, and which will be capable of expression in terms of biological reaction.

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DISCUSSION.

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THERE is no proof at present that the numerous and very varied cases commonly grouped together under the name of dementia praecox are due to one and the same disease process, but such a hypothesis is certainly a very plausible one. The conception of this disease entity is based on what has been noticed in regard to the course the cases run, and is therefore of great help in prognosis. It bears close analogy to that which discovered general paralysis. For some years before that disease was a generally acknowledged entity certain types of mental cases had been noticed to be commonly associated with paralysis.

In mental hospitals we are always on the look-out for cases of dementia praecox, but we do not at present place them under that name in our official records. The name most resembling it is primary dementia, and under this heading we place many of our hebephrenics and a few of our katatonics and paranoiacs. The other cases of dementia praecox of recent origin are to be found under a variety of headings, such as melancholia, mania, stupor, confusional insanity, and delusional insanity, both systematized and non-systematized. The greater number of cases of dementia praecox which have existed for any considerable time are to be found under secondary dementia.

Although the entity is a useful one to recognize, we would like to emphasize some of the objections there are to the name.

Some years ago I began in my official records to call all the hebephrencis and katatonics I could recognize primary dementia, our equivalent for dementia praecox. I soon found that I was getting a large number of recoveries among them, some being, so far as I could tell, perfect recoveries. As the name "dementia" usually denotes a permanent loss of function, it seemed absurd to write down so many cases of dementia as recovered. I therefore ceased to so classify such cases unless the mental weakness was very obvious and I thought likely to be permanent.

Again, as Professor Kraepelin points out, neither dementia nor precocity is an essential feature, and therefore the name is misleading. My own chief objection to the name is the pessimism it involves. It is a principle with most of us to cultivate optimism and try to radiate it out to our patients. But to label an acute case as one which is certain to end in childishness or mere animal existence is most discouraging to both physician and friends. Moreover, such an outlook is not fully warranted by our experience. If we could only find a name which denotes merely a disorder of personality with perhaps a tendency to destroy the brain cells, we could use it freely without committing ourselves to a necessarily hopeless prognosis.

The second question Dr. Hart raised was, What is the nature and causation of dementia praecox? Of the numerous theories indicated by Dr. Hart, I would say three of the combined theories are most in favour at the present time. They are an autointoxication theory, an insufficiency or degenerative theory, and a psychogenic theory. The first supposes an unknown toxin manufactured in the body, and possibly due in many cases to faulty action of the sexual glands. This is the theory most favoured by Kraepelin. The second theory supposes that the individual nervous system has sufficient vital force to last a short life but no longer, and therefore gives way under comparatively trivial adverse conditions. This theory has been very largely held in the past, and quite well suits the rapid cases of simple dementia. It hardly suits the acute recoverable cases.

With either of these theories the indications of dementia, such as loss of emotional and volitional activity and loss of unity and proper combination in the mind processes, are looked on as necessary or fundamental, whereas the hallucinations, delusions, excitement, stupor, feelings of compulsion, impulsiveness, mannerisms, automatic obedience, negativism, stereotypy, etc., are looked on as mostly non-essential, and dependent on the individual mental constitution and its peculiar modes of reaction.

The third theory, based on the teaching of Freud and Jung, supposes the primary cause to be some mental force analogous to that responsible for hysteria, probably a series of complexes, which set up a vicious circle of mental and bodily disorders culminating in more or less permanent loss of cerebral function. This theory also supposes a physical constitution or temperament which exists in some degree in most people, but in a marked degree in predisposed persons. This theory seems to be rather scorned by Kraepelin, on the ground that he cannot conceive how complexes can have such far-reaching results and produce alterations in structure. I think, however, that it is continually getting more adherents, and is the most likely theory to lead to fresh knowledge of the disease.

The ideas and explanations suggested by Freud and Jung which Dr. Hart has set forth are most interesting, indeed fascinating, although Kraepelin would perhaps term them fanciful. I refer especially to the ideas of archaic psychical reactions, narcisstic libido, and excessive introversion.

The third question Dr. Hart raised was, What are the relations of dementia praecox to other disorders? In this connexion it is noteworthy that before admission cases of dementia praecox have often been mistaken for hysteria, neurasthenia, or criminality, and that for some time after admission they are often mistaken for imbecility, manic-depressive disease, confusional psychoses, other delusional states, or even occasionally for epilepsy or general paralysis. Also, it is highly probable that large numbers of the vagrant and criminal class are really mild cases of dementia praecox.

In many mental hospitals there is one medical officer who devotes himself entirely, or almost entirely, to pathological investigations. I am inclined to think we ought similarly to set apart one medical officer almost entirely for psychological investigations, who could give his results to the medical officers responsible for treatment.

C. STANFORD READ, M.D.,
Salisbury.

DR. BERNARD HART has so clearly set out the present position of our knowledge with regard to dementia praecox that there is only necessity to mention one or two points and state where one's personal views tend. Because of the lack of knowledge of definite etiological factors in the production of dementia praecox, the average psychiatrist has been in the unscientific position of endeavouring to define what he means by the term by giving a group of certain symptoms, and, again, when he meets these symptoms he classifies them under the heading of dementia praecox. Such circular methods of dealing with mental disorders have been too rife, and can only be satisfactory to the routinist whose lack of interest is such that he has no desire to seek further for pathological causation. In the light of modern psychopathology the problems as to what extent dementia praecox constitutes a definite entity, and of its relation to other mental disorders, go very much hand-in-hand. Though we find what we may term typical textbook types of dementia praecox constantly, we almost as often encounter anomalous cases which border on and link up with other forms of abnormal mental reactions. In Kraepelin's last work his different types and the separation of a special paraphrenic group, though the product of wonderful observation and clinical acumen, seem to savour greatly of artificiality.

It has been almost entirely through modern psychoanalytic work that abnormal mental reactions have been traced towards their basic roots, and the more this has been done, the more have similar psychological mechanisms been found to have been operative, though varying in degree and modified by other factors. The presence or absence of an hallucination should not tend to create different disease entities, as it is only according to what is dissociated from the personal trend of thought that determines whether it becomes an hallucination or a delusion. In the former the content is an idea which the personality will not accept; in the latter it is accepted, though emanating from without the personal consciousness. The analytic study of paranoia, paraphrenia, and paranoid dementia has tended to show that they are only modifications of the same morbid processes, and it is feasible to assume that further investigation will demonstrate a more intimate connexion with other so-called functional psychoses and the psycho-neuroses. If dementia praecox and hysteria have certain common grounds, my war experience especially has convinced me that there is no hard-and-fast line to be drawn between hysteria and many epilepsies. The work done by Pierce Clark and MacCurdy amply confirms this. By many the chronic alcoholic psychoses are regarded as due to abnormal mental processes of a praecox type, but with alcoholic complications. This, again, tends to unify our psychiatric conceptions. Mental deficiency hardly comes into this category, though careless observers may fail to distinguish between it and dementia praecox, mistaking the faults of inattention, inaccessibility, and poor volition for defects in intelligence—an error which was extremely frequent during the late war. Though, then, dementia praecox may usefully be regarded as a clinical entity, pathologically it does not seem so, and in the near future we shall probably find this confirmed. It seems to me that the bulk of logical evidence at hand is in favour of its psychogenic origin, though, as Dr. Hart says, there are factors which should not allow us to lightly rule out any organic side to the question. The relations existing between mind and body have always hitherto been a bugaboo in mental medicine, but, largely through the study of endocrinology, we are seeing that there is always an integration of the two at some level, and Kempf's work on the autonomic system and the personality has linked up still closer the relations existing between the emotions and their physical correlates.

The analogy between dementia praecox and general paresis seems to give some support to the physiogenic point of view, but in the latter disease the delusional ideas do not have the same special meaning to the individual as on analysis they are found to have in dementia praecox, though of course they in some way relate to the personality. Perhaps one tends to be too sceptical as to the meaning of the morbid histological changes found by some observers, but even if they be accepted the relation of cause and effect is still highly unsatisfactory, though Kraepelin would explain many praecox symptoms on the basis of Alzheimer's findings. Dr. Hart has not brought up the important point of recovery from dementia praecox, nor has he mentioned that psychiatrists of repute report cure through analytic treatment. There seems no doubt that absolute recoveries do take place, and it is difficult to conceive that this could be so if important neuronic degeneration had been the causative agent. Some opponents of the psychogenic theory ask for proof of the functional element by means of a therapeutic test. This, perhaps, is hardly fair for the fully developed psychosis, but evidence may come about through prophylaxis. The deteriorating cause of the disease has been dwelt on as an argument in favour of a physiogenic origin, but one must bear in mind that the deterioration is so often more seeming than real, and that it may only indicate a progressive withdrawal of external interest. In so-called epileptic dementia MacCurdy has shown this to be so, and by successfully re-establishing interest he states that he has eradicated the demential symptoms. I lean, then, towards the view that the essential morbid process in dementia praecox is mainly psychological in origin, and involves through mental conflict a shutting out of reality and a regression to more primitive modes of thought and action. It is in the depth of the regression, the nature of the repression, and the type of dissociation that the various so-called functional psychoses will vary in their symptomatic picture. As Dr. Hart has said, it is probably in a wide biological conception of mental disorder that we shall find our researches most fruitful.

DR. BEDFORD PIERCE (York) stated that little more could be done to arrest the progress of dementia praecox than was possible when Haslam described the condition in 1807. He quoted extracts from Haslam's striking clinical picture

which brings out the characteristic loss of interest in the affairs of life, the emotional episodes, the impulses, and the progress to low-grade dementia. Although modern psychology had done much to explain the mental mechanism of the disorder, it failed to explain why A regresses and B progresses; why A pulls down the blinds, closes the shutters, and cares for no one; why B opens the windows of his soul and rejoices in human intercourse. He mentioned two surprising recoveries which appeared to occur spontaneously, and said the possibility of this had to be remembered when the value of new methods of treatment was estimated. He agreed with Dr. Hart in his suggestion that there might be both physiological and psychogenic factors in the production of the disease, but he could hardly follow him in his suggestion that these might be different aspects of the same process. In the mental changes produced by thyroid insufficiency the psychogenic factors were clearly of small consequence. He could not but think that the physiologist who worked out the consequences of disordered function of the sexual and assessory sexual glands would do more to solve the problem of dementia praecox than elaborate investigation of the mental make-up of shut-in personalities by psychological methods.

Dr. H. DEVINE (Portsmouth) considered that dementia praecox could not be regarded as a definite entity, on the following grounds: (1) The absence of any specific bacteriological or serological tests which aid in the recognition of the disorder. (2) The absence of any definite *post-mortem* criteria peculiar to the disorder. (3) The fact that there are no definite signs or symptoms which mark off dementia praecox from closely allied forms of mental disorder, such as paranoia or paraphrenia. (4) The fact that the characteristic symptoms of dementia praecox are not necessarily significant of a deteriorating psychosis. He suggested that the substitution of the term "schizophrenia" for dementia praecox would be advantageous, as such a term would not of necessity carry with it an unfavourable prognosis. In reference to causation the view was developed that the symptoms of dementia praecox were to be regarded as a constructive effort on the part of a biologically inferior individual to make an adjustment to his internal difficulties. Analysis of cases showed that the defect of adaptation in these cases was mainly in the sexual sphere.

Dr. DAVID BLAIR (Lancaster) desired to support the futility of the label "dementia praecox." Either types of mental disease were included under the title "dementia praecox" which should not, or the disease might be recovered from. He referred to a case which presented well marked katatonic signs for over five years. The patient did not respond to stimuli of pain, heat, or cold; a pin could be inserted in any part of the body or teeth pulled out without response; there were absent knee-jerks and foot-drop, with constant nervous movements of the eyeballs resembling nystagmus. The patient had to be fed for four years, for two of these by the nasal tube. All these signs cleared away, and the patient had been restored to her normal health for quite six years.

Dr. W. H. R. RIVERS, F.R.S. (Cambridge), objected to different theories of origin being regarded as opposed to one another, and believed that every case of dementia praecox, as of other mental disorders, should be regarded from three points of view—the physical, the psychological, and the biological. Of these three aspects, he regarded the psychological as the most important, because it opened the possibility of prevention. It was generally acknowledged that the subjects of dementia praecox showed special mental tendencies in childhood, and it was only through psychological study that these tendencies could be understood and recognized. He hoped that the time would come when a tendency to dementia praecox would be recognized in childhood and education adjusted accordingly.

Dr. HELEN BOYLE (Hove) confirmed what several speakers had said, that quite a number of patients whose illness was indistinguishable from dementia praecox did recover; that an entire change of surroundings and personnel was the factor which appeared the most frequently beneficial, and this quite apart from any definite psychological treatment. Individual attention and the exciting of interest in the outside world was undoubtedly valuable. With regard to early cases, she had at present a small child of 5 in the Lady Chichester Hospital who, if she were older, would be called a dementia praecox.

Dr. DAVID RICE (Norwich) agreed that dementia praecox was a very bad term. To him the word "dementia" implied an actual and irreparable loss of some attribute of mind, and he thought it wrong to use the same term to label very varying groups of symptoms, and that the entity had not been thoroughly established. In his view those who were working under the Mental Deficiency Act ought to be able in time to make clinical observations which would teach them something about the connexion, if any, between mental deficiency and what was now called dementia praecox, so that they might at length have a picture of the subject from birth to adolescence. With regard to Sir Frederick Mott's seeming attribution of degeneration of the sexual glands as the cause of dementia praecox, Dr. Rice did not think this went far enough back, and the question he wished to put was, What brought about those degenerative changes? Whence came the blood change which caused them? It seemed to him that we must go back to the digestive system and ascertain if there was intestinal autointoxication.

Dr. SANDERS pointed out how the discussion was viewed by the inexperienced. Most speakers agreed that many patients suffering from dementia praecox recovered, which they ought not to do if the disease were due to gross degenerative changes. On the other hand, Sir Frederick Mott had satisfactorily shown that there were definite changes in neurons and testes. It must be remembered that his investigations related to very late stages of the disease, and did not preclude the possibility of recovery in cases which had not progressed to such gross lesions; and it would be useful if Dr. Bernard Hart in his reply would discuss this view in connexion with his combined physio-psychic hypothesis. There could be little doubt as to the value in the future of the school doctor, and the discussions in that Section proved that we were again, as usual, learning our duty to revert to the practice of Hippocrates and devote very careful observation of our patients from their first appearance.

Dr. W. D. WILKINS (Cheddleton, Staffs) said that it was difficult to correlate the degenerative changes found by Sir Frederick Mott with the occurrence of cases of recovery, and it would be interesting to hear whether he considered that these changes were absolutely incapable of repair. It had occurred to him that the functions of the disordered neurons might be taken over by other neurons which had not yet been affected, as occurred in cases of re-education of the brain after gross lesions. With regard to the name—which they were all agreed was a bad one—the difficulty was accentuated by the official nomenclature of the Board of Control. If pressure could be brought to bear upon the Board to alter the name "primary dementia" to a more satisfactory term it would be of great service.

Dr. W. A. POTTS (Birmingham) said that it had been suggested in the discussion that dementia praecox might develop on the same lines as congenital mental defect, the two conditions simply being two manifestations of the same essential disorder. If this were so, and it was quite possible, then the cause must often be a toxic one in early fetal life. It was thought now that congenital mental defect was due to the absence of sensitizers, which stimulated development, especially development of the brain. These sensitizers came from the endocrine glands. For instance, in anencephalic fetuses an absence of the cortex of the adrenal gland had often been found. The failure of the endocrine glands was due to a toxin, such as that associated with an attack of influenza of the mother very early in pregnancy. He had been able to trace such a cause in many cases of mental defect. The importance of such infections had been established by the researches of Drs. Orr and Rows. Great differences might be postulated in the damage done by the toxins to the endocrine glands. In extreme cases it might be so great that mental defect was obvious from birth or a very early age. In the less severe there might merely be poor function of the endocrine

glands, which might subsequently be aggravated by psychic trauma or conflict, or by other toxins. In dementia praecox there seemed to be both physical and psychic factors. If this idea of the origin were correct, we must look for prevention, not to the school doctor, but to the ante-natal clinic.

POISONING BY ARSINE.

BY

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HONORARY PHYSICIAN TO THE BLACKBURN AND EAST LANCASHIRE ROYAL INFIRMARY.

IN the BRITISH MEDICAL JOURNAL of June 19th, 1920, Dr. T. H. Wignall described a series of cases of non-fatal poisoning by arseniuretted hydrogen (AsH_3; arsine) occurring during the production of hydrogen gas and the reducing of certain benzene derivatives. Rambousek states that English factory inspectors reported in 1906 three cases occurring in an electrolytic process, and in 1907 two cases—one in a chemist who was separating bismuth from a solution of bismuth chloride in hydrochloric acid, and the other (fatal) in a man who had cleaned out a vitriol tank.

It may be interesting to record a further fatal case from inhalation of the same gas, produced during the manufacture of zinc chloride by treating in stone tanks or "becks," "zinc ashes" and "flux skimmings," residues in the zinc galvanizing process, with commercial hydrochloric acid. Gases are given off freely, and the men are instructed to go away from the beck as soon as the charge has been emptied into it; they are also provided with masks, but state that they do not find them of much use and rarely wear them. When the evolution of gas lessens, more material is thrown in, and towards the end of the process the becks are stirred with long iron bars until the neutralization is complete. In addition to the hydrogen, it is evident from the smell that sulphurous vapours come off, and the following case shows that arsine is also evolved in considerable quantity:

Case History.

J. C., aged 46, on the morning of May 11th, when shovelling zinc ashes into a large beck, complained that he did not feel well. He had been observed leaning on his spade near the beck instead of moving away at once. When he went home at 6.30 p.m. he complained of severe pain in the stomach and of having vomited. Dr. F. W. Taylor saw him late that night. He found marked cyanosis of the ears, lips, hands, and feet. The man complained of shortness of breath, which seemed to be due to pain in the upper abdomen. The next morning the skin was of a uniformly light brown colour and of a peculiar silky smoothness to the touch. The eyes were prominent and the conjunctivae swollen and intensely congested. He was slightly delirious; the pulse was of fair volume and not much quickened, and the temperature was normal. Respiration was quiet. The abdomen, especially the right hypochondrium, was tender, but not rigid; he vomited frequently during the day. No urine was available for analysis, for none had been passed since he came home, and on catheterization only a few drops of blood were obtained. On the 13th he was evidently much worse, being in a state of low delirium, with complete suppression of urine and obstinate constipation, though the vomiting had ceased and he was able to take fluids. I saw him with Dr. Taylor on the 14th. His appearance was as previously described; he was still delirious, taking no notice of anything unless spoken to; the temperature was 98.4°, the pulse 90, of fair volume. Heart sounds were clear, cardiac dullness was increased towards the left, respirations were not markedly quickened. The lungs showed no dullness on percussion, but mucous râles were heard on both sides. He died comatose at 12.45 a.m. on the 15th.

Post-mortem Examination.

On May 16th I made a *post-mortem* examination. The body was well nourished, the skin of a pale olive-brown colour and silky to feel. Lips, nails, and ears deeply cyanosed, eyes prominent, conjunctivae swollen and brownish. The trachea contained a frothy brown fluid, and its mucous membrane was congested; both lungs showed old pleuritic adhesions, and were congested and oedematous, the fluid being brownish in colour. The pericardial cavity contained 1½ oz. of dark brown fluid. Heart enlarged (weight 21¾ oz.); its fat of deep orange yellow colour, muscle and valves apparently normal. The oesophageal mucosa was green, especially at its lower end; the fat of the omentum and mesentery was also of a peculiar green tinge. The exterior and the mucous lining of the stomach and intestines, which both contained a little thick brownish fluid, were of a uniformly slate-blue colour. The liver weighed 79¼ oz., and on section was of a greenish yellow-brown colour; the gall bladder was full of greenish-black bile. The pancreas and adrenals were apparently normal; the bladder quite empty. The kidneys were enlarged (right 8¾ oz., left 7¼ oz.), and on section dark purple-brown. The perinephritic tissues were deeply congested. The spleen weighed 7¾ oz., and was of a deep red-brown colour. The meningeal vessels were rather full, and there was a little clear fluid under the membranes.

Chemical Analysis.

I sent portions of the stomach and its contents, liver and kidney, and some hair from the head, to the Manchester University Public Health Laboratory, and Dr. Henry, who represented the Home Office, obtained and caused to be examined samples of the materials used in the manufacture of zinc chloride. The report is as follows:

The hair contained 5,000 millimilligrams of As_2O_3 per 100 grams; the kidney, 1,000; the stomach, 300; and the liver, 800.

As the liver weighed 2,246.6 grams, it had an As_2O_3 content of 4,537 mmg. On analysing the materials used, it was found that the commercial hydrochloric acid contained 10,000 mmg. per 100 grams; the zinc ashes, 5,000; and the flux skimmings, 15,000. On heating the last named with the hydrochloric acid, a mixture of hydrogen, arsine and hydrogen sulphide was evolved; two cubic inches of arsine is stated to be sufficient to cause death.

The extremely dangerous character of the gas is shown by the fact that the man apparently absorbed a fatal dose before he felt any inconvenience, at any rate not enough to make him move away. Another point of interest is that up to this illness he was stated to have had excellent health, never losing a day's work, yet the heart, liver, and kidneys were all considerably over the normal weight. On section they had the appearance of having been in a hardening fluid, and no blood could be squeezed from them. The very striking light brown colour of the skin, so different from the ordinary jaundice, is also noteworthy.

This is the first fatal case at the chemical works at which this man was employed. I wish to express my indebtedness to my colleague, Dr. Taylor, for permitting me to publish this case, and to Mr. H. Heap, of the Manchester University Public Health Laboratory, for his careful analyses of the material.

A CASE OF ERYSIPELAS TERMINATING IN ACUTE THYROIDITIS.

BY

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ACUTE thyroiditis is a rare condition, but is sometimes associated with infectious diseases such as erysipelas and septicaemia, as explained by McCarrison in his book on *The Thyroid Gland*, in 1917. The details of the following case may therefore be of interest:

Mrs. S., aged about 46, married, with four children, had a swelling in the right lobe of the thyroid since the last confinement, about ten years ago. It had caused no inconvenience, but she had been attending a doctor for some time with beneficial results. On September 23rd, 1919, she suddenly developed erysipelas of the left side of face, scalp, and neck. She was seen by Dr. Duke on September 28th, and was transferred to me on September 30th. At that time the patient was slightly delirious; the temperature was 105°, the pulse 120, and there was marked erysipelas of the left side of the face and scalp and a brawny swelling of the neck. Dr. Beattie was called into consultation on October 1st, and it was decided to continue fomentations to the neck, ichthyol ointment to the scalp and face, and a mixture of quinine and arsenic by the mouth. Anti-streptococcic serum was not given.

The signs gradually subsided in six days, but immediately afterwards commenced on the right side, the temperature rising to 104°. The same treatment was carried out, and the temperature became normal on October 16th.

Three days afterwards small patches of erysipelas appeared on the right buttock, a hard painful, deep-seated swelling could be felt in the gluteal muscles, and the temperature rose to 103°. No fluctuation was obtained, and with fomentations the swelling disappeared, to be followed two days later by the same condition in the left buttock, the temperature rising to 102°; it followed the same course, all signs disappearing by November 2nd.

On November 5th she complained of pain and stiffness in the right side of the neck, and the temperature rose to 101°. The following day the swelling in the thyroid was much larger and very tender and hard; it was adherent to the sterno-mastoid behind and overlapped the clavicle. There was pain on swallowing, and depression of the chin on the sternum on swallowing with marked limitation of chin elevation. On November 8th the patient was hoarse, the swelling was fixed to the skin, and fluctuation could be felt. Mr. Richardson was consulted, an exploring needle was introduced and pus drawn off. An incision was made and a cupful of pus evacuated, which on

examination proved to be a pure growth of streptococcus. The patient made a rapid recovery, but as the sinus would not close Mr. Richardson opened and scraped it, and it eventually healed on February 10th.

During this time the patient was showing increasing signs of myxoedema. The hair fell out so that a wig had to be worn, the body surface became dry, and she easily felt the cold; the pulse was slow and mental depression was present. These signs and the original swelling all disappeared when the sinus closed; marked drawing up of the scar on swallowing was noticed. There must have been a small focus left, for on July 26th she had a rise of temperature and complained of stiffness and swelling over the scar; two days after it was opened, the pus coming away still being pure streptococcus. A vaccine is now being used with the hope of increasing her resistance.

This case is clearly one which, commencing as an erysipelas, developed into a septicaemia and terminated as an acute thyroiditis. Each attack was milder than the preceding one, showing that some immunity was developing. However, I consider in the light of what occurred that had a blood examination been made in the early stages of the illness, an injection of antistreptococcic serum might have avoided the septicaemia and acute thyroiditis.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

HEREDITARY SPASTIC PARAPLEGIA WITH ATAXIA AND MENTAL DEFECT.

THE following are some observations on a family presenting this condition.

The family consisted, in order of birth, of a daughter, M. A., a son, W., and two daughters, M. E. and E. A. There were no other children, and all four were affected. M. A. died in 1905, aged 22; W. died in 1915, aged 27. M. E. and E. A. are alive, aged 28 and 23 respectively. Both parents are alive, aged 60 years, and are unaffected. All the children were born seemingly healthy and intelligent, and went to school in due course, where they learned to read and write. The first sign of any abnormality appeared between the ages of 7 and 8 years, when they were noticed to stagger in walking. This defect progressed, and in a short time they were unable to attend school. The history is clear that in all the onset occurred in the same manner, and at approximately the same age. The two survivors, M. E. and E. A., present practically the same signs, so that one description will serve for both.

Mentality.—Expression vacant. Speech very slow, and articulation clumsy. Can answer simple questions, such as name and address, and can repeat alphabet, but cannot read. Able to form some of the letters in their names in slow, tremulous writing.

Eyes.—Double internal strabismus, lateral nystagmus; pupil reflexes normal.

Reflexes.—Greatly increased knee-jerks, ankle clonus, Achilles and triceps jerks. Babinski's extensor response in left foot of E. A.

Feet.—No marked deformity, slight tendency to pes cavus and talipes varus.

Gait.—On walking, the toes drag and the feet are spread out, but they cannot walk more than a yard without holding on. They often fall and hurt themselves, but there are no fits. With eyes open, they are unable to stand without swaying, and if the eyes are closed they tend to fall immediately.

Tremors.—Slight intentional tremors.

Menstruation.—Both commenced menstruation at 13, but after three "periods" the flow ceased completely, except in E. A., who had a slight "show" in April, 1919.

The palate in both is well formed, and there is no spinal curvature.

Although the condition itself is well known, and is believed to be allied to Friedreich's ataxia, yet its hereditary origin is difficult to elicit, and this case presents no exceptions. As stated above, both parents are alive and unaffected. On the mother's side, no abnormality can be traced. The father is the illegitimate son of a woman who was an inmate of Wigan Workhouse owing to blindness following an accident to her eyes at a mill, and of a man who was an "invalid" in the workhouse. The nature of this invalidity is not known, but it is known that he left Wigan and died in Preston Workhouse Infirmary, not long after this son was born. As this man died in 1860 or 1859, it is hardly likely that the records of either Wigan or Preston Workhouses would reveal much as to whether he suffered from a chronic degenerative disease of the central nervous system or not. What is certain is that this man suffered from some affection which made him a chronic invalid in a workhouse infirmary.

Now, the woman who gave birth to this illegitimate son—the father of the persons affected and described in this note—was herself apparently free from any transmissible defect, because before becoming blind through an accident, she gave birth to another illegitimate son by a different father, and this son is still alive, and has a healthy family of four children. This reproductive test clears the woman of transmitting the defect, and leaves it to be inferred that the only traceable origin would presumably lie with the invalid grandfather, who through the medium of his unaffected son passed it on to his grandchildren.

Although the evidence as regards heredity is not quite clear, yet it would be still more fallacious to presume either syphilis or alcoholism in the parents, of which there is no evidence at all.

Warrington. J. S. MANSON, M.D.

A CASE OF LOBAR PNEUMONIA ACCOMPANIED BY ACUTE APPENDICITIS.

THE following case shows that acute abdominal pain accompanying lobar pneumonia may sometimes be due to a simultaneous attack of acute appendicitis. The textbooks so often warn the student to distinguish carefully between the two diseases, that it is well to remember that they may in rare cases coexist:

M. S., aged 5 years, was under my care for an attack of bronchitis when on April 26th, 1920, I was called to see her owing to the onset of acute pain in the abdomen. On arrival I found her in great pain, having rigidity of both recti and being very intolerant to examination. Her temperature was 101.6° and her pulse 130; respirations were 36. Examination of the chest showed dullness on percussion at the base of the right lung, with inspiratory crepitations. Her face was symmetrically flushed and the tongue coated with a brownish fur. She had vomited once. On the following day there were unmistakable signs of a right basal lobar pneumonia, and the temperature chart for the next few days was typical of that disease. The abdominal pain gradually subsided, but a week later there were still localized pain and rigidity in the right iliac fossa. The temperature remained above normal, and became oscillating in character; the pulse varied from 90 to 100. She was removed to the Isle of Wight County Hospital for observation on May 5th. The temperature and pulse became normal, but even after all signs of consolidation of the lung had disappeared there remained still slight tenderness and resistance in the appendix region. On May 17th appendicectomy was performed by Dr. Turner, assisted by Dr. Pridmore. The appendix was found to be inflamed; the tip had sloughed off, and a concretion was found in the peritoneal cavity. Numerous adhesions in the appendicular and caecal regions made removal of the appendix difficult; no abscess existed round the concretion. The child made an uninterrupted recovery, and is now quite well.

It is remarkable that in spite of the acuteness of the appendicitis (as shown by necrosis of the tip of the organ) no abscess was found at operation. There was no history of a previous attack of abdominal pain. The question arises whether the two inflammatory processes arose simultaneously from a common septic focus, or whether one was secondary to the other. I am indebted to Dr. Turner for his courtesy in supplying me with details of the operation, and to Dr. Pridmore (in whose practice the case occurred) for his assistance and advice.

Ryde, I.W. L. FIRMAN-EDWARDS, M.B., B.Ch.

INFLAMMABLE GAS AS A PRODUCT OF FERMENTATION IN MALIGNANT DISEASE OF THE STOMACH.

As there are but few recorded cases of this nature, I think the following is worthy of being added to the list and may prove of interest to readers of the JOURNAL.

A middle-aged man had suffered for three weeks from symptoms of indigestion, and complained of foul eructations of wind, to which he was becoming very sensitive. He had not suffered from vomiting or pain, but had lost appetite and weight, and his bowels were confined. His abdomen was distended and "ballooned." I suspected the presence of tumour formation, but this was not palpable. One evening after supper when lighting his pipe he expelled a quantity of wind, which, much to his alarm, caught fire and singed his eyebrows and moustache. The eructed gas was not always inflammable, but on two occasions I was fortunate in seeing its inflammability demonstrated. The gas burned with a pale blue luminous flame and an explosive report; it was probably marsh gas. With the subsidence of fermentation under treatment a tumour of the stomach became palpable, from which he died a few months later.

Glasgow. W. H. STIRLING ARMSTRONG, M.B., Ch.B.

Reviews.

THE PROFESSIONS IN A SOCIALIST COMMONWEALTH.

MR. AND MRS. SIDNEY WEBB have published a book entitled *A Constitution for the Socialist Commonwealth of Great Britain*,[1] which is a presentation of the case for industrial socialism, its consequences being displayed in a scheme for a new constitution. The first object in view is the abolition of capitalism and its replacement by State and municipal production and distribution. The scheme proposes two parliaments—a political parliament concerned with external relations and with justice, and a social parliament to exercise national control over the nation's economic and social activities. It, and not the political parliament, would be the national financial authority, fixing prices, making taxes, if any, and controlling expenditure. It would do its work through a large number of special standing committees, with a staff for administration and another for criticism and research. Each standing committee would appoint a national board, with full power over administration in its widest sense, subject only to the control of the social parliament. These boards, it is suggested, should consist as to two-thirds of principal officers of the service and representatives of the vocations to which the bulk of the employees belong, the remaining third representing the interests of the public. But it is not suggested that industrial administration should be centralized to the extent the above statement might seem to imply. There would be district councils, nominated by the national board, and works committees. The general tendency of the scheme to magnify local government at the expense of the State will at the onset dispose many in its favour. It proposes the universal provision by local authorities of

> water, gas, and (so far as its distribution is concerned) electricity; of such local transport as tramways, omnibuses, ferries and river services; of the provision of houses of all sorts; of public baths and washhouses, and burial grounds and crematoria; of complete sanitary services, from paving, cleansing, and lighting the thoroughfares up to every kind of preventive and remedial treatment for the sick, at home and in institutions; of a complete provision for the special needs of maternity and infancy, infirmity and old age; of education of every kind and grade, from nursery schools to post-graduate courses available for students of any age; of parks and open spaces, woodlands and mountain sides, with holiday homes of all sorts.

It may be noted incidentally that it is proposed eventually to make use of the country houses for this last-named purpose. Under the capitalist system, it is said, the government of industry is vested in the hands of a relatively small fraction of the community—namely, the private owners of the instruments of production. This is the nearest approach we have found in this book to a definition of capitalism. The argument throughout rests on the assumption that capital, at any rate capital in private hands, is an unmitigated evil. In a fully socialized State, though individuals would be able to save, and even encouraged to do so, the State and local authorities would pay for everything they did out of the receipts from the nationalized and municipalized industries. Municipal borrowing is held to be an evil, and unnecessary. A man, or a family, would be encouraged to own a homestead and to save as much as £10,000 (at present values presumably); but beyond that amount capital in individual hands would be expropriated gradually—partly by the local authorities taking over more and more industries, and partly by heavy direct taxation and death duties. The general result aimed at is a uniform standard of living for all workers, whether with the hand or the brain. After a period of transition the social commonwealth would find its national and local resources accruing almost entirely from national and municipal industries, with, perhaps, something from some services. The prices at which the products of the national industries and services would pass into consumption or use would be determined by the social parliament on the recommendation of its standing finance committee.

It is recognized by the authors that there must be some independent check on the influence of the secretarial bureaucracy and its salaried technicians. This check, they hope, would be provided by independent experts in the several professions. In the case of medicine, for instance, the municipalities would provide free to everybody a complete medical service for domiciliary and institutional treatment, but both the individual and the municipality would be free to consult an independent expert, who would criticize the recommendations of the municipal expert or present alternative proposals. Exactly what the inducement to become an independent expert would be is not made clear. While it must in justice be admitted that it is extremely difficult to think out the consequences of the proposals, the absence of definition of terms used here and in other parts of the book is not so easily to be excused. The authors are impressed by the ethical standards which professions, and in particular the medical profession, have set up, and desire that the trade unions shall follow their example in this respect, and also in the voluntary organizations the professions create, such as the British Medical Association, for the purpose of promoting science and knowledge.

The aim of the authors seems to be to devise a socialized commonwealth which shall be self-sufficing; but here, again, the consequences are not adequately thought out. As the socialist commonwealth gets into full working order the yield from direct taxation in the form of income tax or death duties must rapidly diminish until the vanishing point is reached. There are passages in the book which seem to imply that foreign trade must grow smaller as the ideal of the industrial commonwealth is approached. At the same time the scheme contemplates a very large number of paid officials, through whom the nation's will is to be ascertained or carried out. Municipal councillors, for instance, would receive a salary and give their whole time, and the cost of the staffs of central and local authorities for the huge administrative scheme, with its checks and counter-checks, must tend to increase. It would seem that the scheme contemplates that the money necessary to balance the budgets of the social parliament and municipalities would in course of time be derived altogether, or very nearly altogether, from the receipts from the national and municipal industries and services. The net proceeds would depend upon the fees or prices charged to the consumers, and those prices therefore would have to be fixed at such rates as would provide a revenue for the State or the municipality. It is not easy to distinguish this from indirect taxation.

The authors would agree that in the application of their scheme many difficulties would be encountered. One that seems to go to its root is that this country does not provide food for its present population; full data on this point were worked out during the war, and the authors would have done well to refer to the calculations and conclusions published in the reports of the Royal Society's Committee. Nobody seriously supposes possible an increase of production of food in anything like the proportion that would be necessary. The authors have no doubt considered this question, but they do not give their answer. There seems to be only two alternatives—a large decrease in population or the continuance of foreign trade on much the same scale as now. It is fairly easy to conceive the State selling abroad part of the produce of the nationalized coal mines; the price would be determined, not by the social parliament, however, but by the world demand and supply. It is not so easy to imagine a municipality buying abroad cotton and leather and selling abroad cotton sheeting and boots and shoes at prices adjusted, not to supply and demand abroad, but to the needs of the municipal exchequers.

Some readers may be disposed to think that too much space has been given to this book; but it is said to be having a wide circulation in labour and trade union circles, and is intrinsically instructive, since it may be supposed to show the ideal of the socialists belonging to the school dominant in this country. It would seem that the authors have formed so high an estimate of the ethical standard and intellectual alertness and altruism of the scientific professions that they have been unwilling to think out the consequences of their proposals; were their scheme carried out the scope for the independent practitioner could not from the first be large. Everybody would be entitled to the municipal medical service, and within a generation there would be no rich. The account given of the extent

[1] *A Constitution for the Socialist Commonwealth of Great Britain.* By Sidney and Beatrice Webb. 1920. London: Longmans, Green, and Co. (Demy 8vo, pp. 382. 12s. 6d. net.)

to which socialized production has gone is interesting, and the reader feels that the authors are here discussing a subject with which they are well acquainted. The general implication seems to be that all productive trades should be carried on by or through the municipality, but the warm praise of the co-operative movement leaves the reader a little in doubt as to what the expectation actually is. The writers "realize the imperative need for a rapid development of science in its widest sense, alike in the discovery of new knowledge and in the universal dissemination of the scientific methods of thinking"; yet the whole tone of the book is materialistic. Man's intellectual side and his spiritual aspirations are hardly taken into the reckoning; yet they form the greater part of him and determine his conduct, since that is not, save to a small extent, determined by instinct.

CUNNINGHAM'S ANATOMY.

CUNNINGHAM'S *Manual of Practical Anatomy*,[2] of which the seventh edition has just made its appearance, must be so well known to all medical students and medical men that anything in the nature of a detailed description would be an act of supererogation on the part of the reviewer. The work increases in size with each edition, so much so that the publishers have considered it advisable to publish the present edition in three volumes, reverting, if we are not mistaken, to the same number of volumes as that in which the first edition was issued. The resemblance, however, between the first and last editions stops largely here, for in the length of the text and in the number and character of the illustrations it would be difficult to find two editions of a book more unlike. The difference is probably to be associated with two important changes in the teaching of anatomy—the increasing difficulty in obtaining material for dissection, and the increasing claims made by the teachers of other subjects on the time of the medical student. The result is that, even if he had the opportunity, the student has not the time to devote to dissection as formerly, and is forced by stress of circumstance to acquire much of his anatomical knowledge from reading and from the study of drawings. To such students Cunningham's *Practical Anatomy* must surely supply everything they can require; in fact, along the lines on which it has evolved it seems to us as perfect a production as can well be imagined. The only criticism we can accept regarding it, is that its length is such that only a very small proportion of students can obtain in the time available a satisfactory knowledge of its contents. The capacity of the medical student has not, unfortunately, kept pace with the expansion of the book.

The present edition runs to some 500 pages to each volume of closely printed text, lightened, however, with numerous illustrations; in fact, the illustrations are becoming more and more the feature of the book. There are no less than 18 different illustrations of the anatomy of the heart, apart from figures in which the heart enters with other structures and apart from two radiographs of the pericardium. To the description of the brain 150 pages are assigned and 70 illustrations, to the description of the ear 20 illustrations. The result of this profuseness in illustrations is undoubtedly to make the study of the subject more easy and to facilitate reference and revision. The editor, Professor ROBINSON, has, in the present edition, introduced several new features and extended others, all distinct improvements; he has, for example, put the instructions for dissection in distinctive indented type, which at once arrests the student's attention; window dissections of the pleura and peritoneum are shown in such a way that the minimum disturbance of structures is made; radiographs of various parts of the alimentary canal after a bismuth meal are given, and special attention is paid to such parts as the stomach, duodenum and prostate, in view of their importance to the surgeon.

Altogether, as an exhaustive treatise on practical anatomy the book is far in advance of any other with which we are acquainted, either in English or in any foreign language.

[2] *Cunningham's Manual of Practical Anatomy.* Revised and edited by Professor A. Robinson. Seventh edition. Volumes II and III. London: Henry Frowde, and Hodder and Stoughton. 1920. (12s. 6d. each volume.)

ARTERIO-SCLEROSIS AND HYPERTENSION.

THE second edition of Dr. WARFIELD'S monograph on *Arterio-sclerosis and Hypertension*[3] appeared eight years ago, and the third edition contains much new material treated with refreshing independence and sound common sense; but though the opinions expressed are the author's, it is obvious that he has a wide knowledge of the literature of the subject. High blood pressure is always a compensatory process, and should, the author points out, be regarded in the same light as a compensated cardiac lesion. The blood-pressure picture consists of the systolic, diastolic, pulse pressure and pulse rate taken together, and not of isolated estimations of the systolic pressure. The diastolic pressure is the most important element in the picture, for a persistent high diastolic pressure means increased work for the heart and arterio-sclerosis. The full significance of the pulse pressure—namely, the difference between the systolic and diastolic readings—is explained more clearly and succinctly than we remember to have seen it stated elsewhere.

The new chapter on blood pressure in its clinical applications begins with the timely warning that blood-pressure estimation is one only of many methods of acquiring information, and that he who worships his sphygmomanometer as infallible will sooner or later come to grief. The value of a falling blood pressure in surgical operations as the earliest sign of approaching shock is pointed out, and the value of a rising pressure in pregnancy as evidence of toxaemia is emphasized. Dr. A. F. Hurst's view that the vomiting of pregnancy is hysterical appears to be supported by Dr. Warfield's dictum that although emesis gravidarum is supposed to be a sign of a toxaemia of unknown nature, the blood pressure is never raised, even in the pernicious form. The cases of high blood pressure are divided into three groups: (1) those of chronic nephritis with cardio-renal death; (2) essential hypertension, or hereditary, with little clinical evidence of kidney disease and death from cerebral haemorrhage; and (3) arterio-sclerotic with cardiac death.

Arterio-sclerosis is not regarded as a disease with a definite etiological factor, but as a degenerative process due to various causes which are considered seriatim. It is divided broadly into two—the congenital, or the result of inherited tendencies, and the acquired forms. The importance of absorption of the products of perverted intestinal metabolism in causing a high blood pressure, and so arterio-sclerosis, is recognized, but until more convincing evidence is forthcoming the view that the adrenals, thyroid, and pancreas are concerned in the production of arterio-sclerosis is not accepted. In the opinion that oarsmen show a relatively high mortality at the different decades, traceable to the raised blood pressure produced while in training, Dr. Warfield differs from the conclusions reached by the late Dr. J. E. Morgan in his work *University Oars* (1869). Probably most readers will agree that cigars are more likely than cigarettes or pipes to induce arterio-sclerosis. Prophylaxis may be summed up in the plea for a more rational mode of life, with moderation in all its duties and pleasures. In order to recognize the disease in its early stages, people over 40 years of age should get into the habit of being overhauled by the doctor twice a year, in the same way that most people pay periodic visits to the dentist. This attractively written volume ends with a chapter of epigrammatic practical suggestions which sum up the lessons already given.

NOTES ON BOOKS.

The Revels of Orsera,[4] by Sir RONALD ROSS, is a weird, fantastic book, justifying its subtitle, for it is both mediaeval and romantic. The scene is laid in the Alpine valley of Andermatt and amongst the peaks which lie around it, at the end of the fifteenth century, the chief events centring in the proposals of sundry suitors for the hand of Lelita, daughter of Count Reichenfels, the local magnate. We have here all the usual paraphernalia—tournaments, feasts, and the rest. These are well described, though with no very marked originality, and the

[3] *Arterio-sclerosis and Hypertension.* By Louis M. Warfield, M.D., F.A.C.P., Chief Physician to Milwaukee County Hospital. Third revised edition. London: Henry Kimpton. 1920. (Med. 8vo, pp. 265; 60 figures. 24s. net.)

[4] *The Revels of Orsera: A Mediaeval Romance.* By Sir R. Ross. London: J. Murray. 1920. (Cr. 8vo, pp. 399. 7s. net.)

author skilfully uses the setting—the high mountain air with its sudden weather changes—to give a special colour to the events. To draw even an outline of the plot beyond the very general one already mentioned would be difficult, as the more ordinary circumstances are affected by the semi-miraculous doings of certain dwellers in the high valleys. The manner in which these personages (mountaineers from their youth though they certainly are) go gaily up and down the mountains moves us, we confess, to admiration. The characters are all drawn with a firm hand, and to two of them special praise may be given—to Zozimo, the hunchbacked poet with his great soul in the feeble body, and to Gangogo the jester. The latter strikes us as of great merit—ardent, peevish, mischievous, philosophizing, witty, and pathetic, with just the right mixture of wisdom and folly—he holds our interest from his first appearance to his tragic death on the mountain. In recommending the book to our readers we need only add that the narrative runs in easy periods, both in description and dialogue, and that the writer knows well and pictures forcibly the grand scenery that forms the setting of his romance.

The pleasantly-written pamphlet, *What to tell our Grown-up Sons about Women*,[5] by the Widowed Physician, is much shorter than the same author's *Letters to his Daughter*, but resembles it in the soundness of its advice. It is perhaps as well to mention that the advice is mainly directed to the means of judging the character of the opposite sex, and is not, as possibly might be thought, devoted to what is commonly understood as sexual relations. The behaviour and attributes of the female in the successive stages of the infant, the child, the girl, and the mature woman are passed in review, and the dangers to be borne in mind are summarized in twelve warnings. The first is: "Beware of the girls who manicure their nails to the shape of a claw. I do not know why, but beware of them." The second has an ancient flavour, but is none the less wise for that: "Beware of girls who prefer to dress in purple or scarlet colours. There is usually something wrong with their morals." And the eleventh, "Beware of the girl who drinks wine freely; she will make a poor wife and a worse mother," is noteworthy if only for the double meaning that may fairly be read into the adjective "poor."

Dr. DOUGLAS COW'S *Synopsis of Pharmacology*[6] is a brief cram-book for medical students, compiled in the form of a dictionary, and with as few of the general vices of the cram-book as any of its class we have set eyes on. It is designed for use in conjunction with one of the more extensive and reasoned textbooks of pharmacology, and contains a great deal of well chosen and well arranged information. The author suggests that it may be of service to the student for perusal in the usually crowded days preceding an examination, and deprecates the idea that it should be used for cramming—a happy combination, if one may say so much, of the harmlessness of the serpent with the wisdom of the dove.

In the third edition of *Surface Anatomy*[7] Mr. C. R. WHITTAKER follows along the same lines as in former editions, confining his attention to a brief and simple statement of well-established facts. The book is primarily for the use of students preparing for their final examination, and is, we think, well adapted to serve the purpose for which it is intended. The use of marginalia facilitates reference, but we think the author should come to a definite decision as to whether they are to be part of or apart from the text; sometimes—as on page 3—they are one, sometimes the other. The figures illustrating cranio-cerebral topography present a distinctly unusual form of Sylvian fissure, while the sutural lines in the naso-orbital region are grossly inaccurate. Perhaps in future editions—for which we imagine there will be a demand—the suggestion of giving profile views of the various regions in addition to the conventional front and back views will be considered. We think, too, that the introduction of a few carefully chosen radiographs would add to the attraction and value of the book.

[5] *What to tell our Grown-up Sons about Women.* By the Widowed Physician. London: The St. Catherine Press. 1920. (Pp. 23. 1s.)

[6] *Pharmacology.* By Douglas Cow, M.D. Students' Synopsis Series. London: J. and A. Churchill. 1920. (Cr. 8vo, pp. 132; 15 figures. 7s. 6d. net.)

[7] *Surface Anatomy.* By Charles R. Whittaker, F.R.C.S.Ed., F.R.S.E. Third edition, revised and enlarged. London: J. and A. Churchill. 1920. (Demy 8vo, pp. 88; 27 figures. 7s. 6d. net.)

MESSRS. J. AND A. CHURCHILL announce for early publication an illustrated textbook on preventive medicine

APPLIANCES AND PREPARATIONS.

A Pipette for the Automatic Measurement of Constant Volumes.

CAPTAIN W. HENWOOD HARVEY, M.D., R.A.M.C.(T.), Officer in Charge, Central Military Laboratory, Cambridge, writes: The pipette here described has been designed for use in such routine work as complement-deviation tests, in which a series of equal volume measurements of different reagents may be required.[1] The construction is most readily explained by reference to diagrams.

In Fig. 1 the instrument is seen to consist of two tubes, one fitting over the other, united by a rubber bung. The outer tube or reservoir, A, is made from a piece of moderately heavy glass tubing, 10 cm. long and having an internal diameter of 1 cm. The upper end of this reservoir is sealed off at S, and the lower end is slightly flared at O. At a distance of 1.5 cm. from the sealed end, and at right angles to the main tube, a small tubule, B, is set off. This tubule is 1.5 cm. in length, and has its free end finished for use with a rubber teat. A second tubule, C, 4 cm. long, is set off, also from the main tube, and at right angles to it, at a distance of 2 cm. from the lower and open end, O. These two tubules are not placed vertically to each other, but form an angle of 90 degrees, as seen when the pipette is viewed from either end, as shown in Fig. 2. In this figure the relative position of these two tubules to each other is represented in full outline for a right-hand pipette and in dotted outline for a left-hand one. The inner tube or measuring chamber, E, is made from a piece of glass quill tubing, 20 cm. in length, and of small bore. Each end of this chamber is drawn off to a suitable taper for the delivery of fluids. This tube is fitted with a single-hole rubber bung, D, which serves to connect it with the outer tube or reservoir. It should be so adjusted that its upper end reaches well above the level of the tubule, B, when the pipette is held in an upright position.

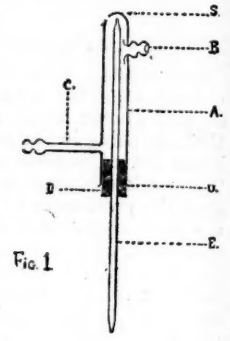


Fig. 1

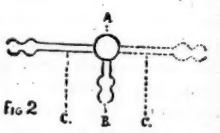


Fig. 2

From the above description it will be seen that the measured volumes are entirely dependent upon the capacity of the measuring chamber, and that, provided care is taken to fill completely and empty completely this chamber at every charge and discharge, the volumes will be the same. By varying the size of the inner tube a corresponding change may be made in the measured volumes. If it is desired to work with comparatively large volumes, such as 2 c.cm., it is advisable to use a pipette having the outer tube or reservoir made from a larger size of glass tubing than that specified in the foregoing description, in order to admit the large measuring chamber required for such volumes and yet ensure a sufficient holding capacity for the excess which is drawn up at each measurement.

For use, a rubber teat is fitted to the tubule B and a length of moderately thick-walled rubber tubing, closed with a pinch-cock, is connected to the tubule C. This tubing may be sufficiently long to be used as a flexible coupling between the pipette and an exhaust pump, which arrangement will facilitate emptying the reservoir and washing the instrument when required. Hold the pipette vertically in the hand in such a way that the tubule C rests high up in the cleft between the fourth and third fingers, and the main barrel is grasped firmly by the fourth, third, and second fingers. The teat will then be found in a convenient position for manipulation with the first finger and thumb. To charge the pipette, compress the teat, immerse the point of the measuring chamber in the reagent, and release the compression by removing completely from the teat the finger and thumb. The measuring chamber then will become fully charged, and the excess drawn up will overflow into the reservoir, from which it may be reclaimed or wasted through the tubule C. Still holding the pipette upright, carry it over to the tube intended to receive the charge, insert the point of the measuring chamber as far down the tube as desired, and discharge the contents by a steady recompression of the teat. With a little practice it is easy to judge the requisite amount of compression necessary fully to charge the pipette so that only a very minute quantity of the reagent overflows into the reservoir.

A number of pipettes, measuring different volumes and made in accordance with these specifications, were tested throughout a series of over four thousand routine Wassermann tests, and were found to give consistent results.

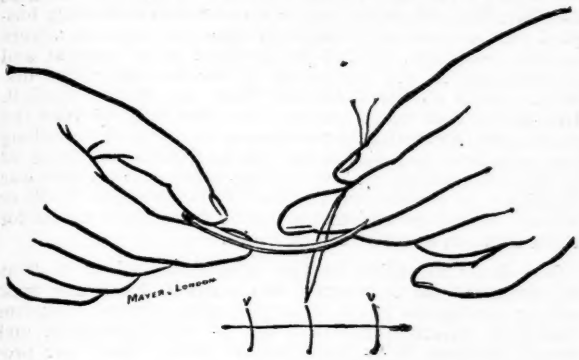
[1] This pipette may be obtained from Mr. S. Rampling, surgical instrument maker, Regent Street, Cambridge.

By using two such pipettes, fitted with different sized measuring chambers, a ready and rapid means is provided for preparing a series of dilutions of any given reagent. The instrument is easy to make; it is simple, economical, and reliable in use; it is entirely independent of any time factor during usage. Whilst discharging its contents into a tube, it delivers with sufficient force to mix thoroughly the reagents present, and yet avoids splashing the sides of the tube.

I desire to acknowledge my indebtedness to my former assistant, Dr. Florence Scott Kirk, for the help she gave me throughout the many trial tests of these pipettes and in the demonstration of their use, which was given at the meeting of the Pathological Society of Great Britain and Ireland, held in Cambridge on June 30th, 1920.

A New Surgical Needle.

Mr. E. M. COWELL, D.S.O., F.R.C.S. (London, W.), writes: I have designed a needle with a knife edge for use in suturing skin, fascia, or muscle. By virtue of its knife edge it may be used to cut the suture material employed, and thereby save the time otherwise wasted by using scissors. In



putting in interrupted stitches, the knots are tied with the left hand, and the right is used to cut the stitch, as shown in the figure. The needles are made straight or curved. It is easy to keep the knife edge keen by rubbing it on an oil-stone. The instrument is made for me by Messrs. Mayer and Phelps, of 61, New Cavendish Street, W.1.

THE INFLUENZA EPIDEMIC IN NEW SOUTH WALES.

THE New South Wales Department of Public Health has issued a very full report on the influenza epidemic of 1919 as seen in that colony. We gave on July 31st, 1920 (p. 185), a summary of the mortality statistics. The population of New South Wales is two millions; in Sydney, the capital and principal seaport, 36 per cent. of the population of 800,000 were attacked, and 3,902 persons died—a case fatality rate of 1.3 per cent. Dr. W. G. Armstrong, the Deputy Director-General of Public Health, points out that the comparatively late appearance of the pandemic in Australia and the isolated position of the continent gave the Commonwealth an unusually favourable opportunity of attempting to hold up infection by measures of quarantine. England was first invaded in May, 1918; America, as well as South Africa, in September, and New Zealand in October. During October vessels arriving in Australia from Africa and New Zealand were quarantined for several days; the first influenza case arrived from New Zealand on October 25th. For three months the quarantine was attended with success, but in January Melbourne became infected, and from there Sydney and all parts of Australia. In New South Wales energetic measures were taken to check the spread of the disease. The report states that notification, isolation of patients and contacts, prohibition of assemblies, compulsory masking in public places and severe restrictions on travelling, together with prophylactic inoculation, checked for a certain time the development of the epidemic and afterwards prevented it from assuming the explosive character seen in many other countries.

Prophylactic Inoculation.

For the prophylactic inoculations a mixed vaccine was used, obtained from patients in quarantine; for the most part its composition was: Pneumococcus 1,000, influenza bacillus 100, streptococcus 100, *Staphylococcus aureus* 250, Gram-negative micrococci 250 million respectively per cubic centimetre. An initial dose of ½ c.cm. was followed in seven days by a dose of 1 c.cm. No charge was made, and between 35 and 40 per cent. of the inhabitants of Sydney and nearly 25 per cent. of the population of the whole State were inoculated. It was not possible to collect evidence of the effect of inoculation on the liability to be attacked, but the mortality appears to have been distinctly lessened. Among 13,000 persons treated for influenza in the public hospitals of Sydney, the mortality in the uninoculated was 16.5 per cent., compared with 8.2 per cent. in those who had received three prophylactic injections. Some doubt is expressed as to whether the protection conferred was really specific, or whether the vaccine had a general "alterative" effect due to the injection of foreign proteins. Notification and isolation of patients had little value except in the early stages of the epidemic; the control of travelling was found to be useless, but prohibition of assemblies is thought to have been attended with some success. The evidence of the Sydney epidemic was against the suggested spread of the epidemic by "carriers"; in no case was infection observed to be conveyed by those who had been discharged from quarantine while still harbouring Pfeiffer's bacillus in their throats. A study of the cases of influenza occurring in the hospital staffs show that one attack of influenza appeared to afford a high degree of immunity, which was not, however, very lasting. Among 1,488 members of these staffs 814 were attacked by influenza, but of these only four subsequently suffered from a second attack. Although the figures are small, there is some evidence of a special degree of susceptibility among pregnant women; among 224 pregnant women treated in hospital the mortality was 26.7 per cent.; mortality rates of 8.0 and of 14.9 per cent. were observed among the much larger totals of females treated in hospital at the ages of 20 to 29 and 30 to 39 respectively.

Bacteriology.

Dr. J. B. Cleland, principal microbiologist to the Department, contributes a comprehensive report on the pathology of the epidemic. From bacteriological studies he is inclined to believe that the primary etiological agent of influenza is at present unknown, but is probably a virus of the filter-passing variety. In fatal pulmonary cases organisms of one or more groups were usually present in considerable numbers; that most frequently found was the pneumococcus, but the influenza bacillus, streptococcus, and *Staphylococcus aureus* occurred, each in about one-third of the cases. Dr. Cleland regards these bacteria as secondary invaders flourishing in the exudate or haemorrhagic areas resulting from the action of the primary virus in the lungs. Animal inoculations with blood or with material from the lungs, sputum, saliva, or naso-pharyngeal secretion of influenza patients, led in every instance to negative or inconclusive results; monkeys and baboons were found in forty experiments to be refractory to the disease. A description of the *post-mortem* findings in 130 cases is illustrated by a number of fine coloured plates.

Conclusions.

In his prefatory letter Dr. Robert T. Paton, the Director-General of Public Health, pays a tribute to the untiring services rendered by the staffs both of the ninety-two emergency and of the pre-existing hospitals; he mentions also the valuable services rendered to the Department by a Consultative Committee of the British Medical Association formed at the request of the Government. Dr. Paton concludes that the most potent factor in the saving of life appears to have been the institution of early treatment. He believes that the wearing of well-fitting masks by persons brought into contact with cases was of considerable value, and he has no hesitation in advising inoculation and re-inoculation at fairly short intervals should the State again be threatened with an invasion approaching in virulence that of 1919. For this purpose large quantities of vaccine are being prepared.

THE Jewish Medical Society of Palestine is publishing a quarterly journal entitled *Harefoeah* (Healing); the first number is dedicated to Jewish medical men who were killed in Palestine during the war.

HISTORY OF ASYLUM WAR HOSPITALS.

THE valuable and interesting report to the Home Secretary on Asylum War Hospitals in England and Wales, by Sir Marriott Cooke and Dr. Hubert Bond, Medical Commissioners of the Board of Control, to which we made brief allusion in our issue of September 11th, is worthy of the attention of our readers and the public generally, since more than one-sixth of the total number of sick and wounded men from all fronts during the war, or 482,949 officers and men, were treated in these hospitals.

It is, perhaps, necessary to ask, even in these times, what the Asylum War Hospitals are, or were before they sprang into being. They were twenty-four out of the hundred or so public mental hospitals of England and Wales, or in still surviving legal phraseology, the County and Borough Pauper Lunatic Asylums, or, as the novelist and sentimental crank would term them, the madhouses of the country. That they should, with varying degrees of conversion, according as they were old or up-to-date institutions, have been easily and rapidly converted into first-class general medical and surgical hospitals, which won the highest praise from the Medical Department of the War Office, from highly qualified civilian critics, and from the officers and men treated therein, may be surprising to those unacquainted with the buildings, organization, and resources of a public mental hospital.

How this was effected is fully described in the report; in terse and clear exposition Sir Marriott Cooke and Dr. Hubert Bond describe how the Board of Control came to meet the dire need of the War Office for hospital accommodation in the early months of 1915 by a scheme the practicability and soundness of which soon won over the support and collaboration of all parties concerned.

England and Wales were divided into areas, and one asylum in each area selected as a war hospital and the mental cases therein evacuated into the other asylums in that area. Thus were 15,000 beds almost immediately available, till eventually, by further development of the same scheme, over 31,000 beds were provided, in which 482,949 officers and men were treated. At this time some well-meaning but technically ignorant and ill-informed persons, both inside Parliament and outside it, protested against our sick and wounded heroes being located in "pauper lunatic asylums." It may be asked, Where else could nearly half a million sick and wounded have been cared for? A perusal of the report will furnish an answer. The military hospitals framed on a peace basis were inadequate to deal with such numbers, the Territorial hospitals had to be built and organized *de novo*, and, excellent as they were, consisted mostly of temporary buildings which could not compare in resources and amenities with going concerns like the asylums with their "skeleton" medical and nursing staffs, wards, laundries, kitchens, bakeries, engineering and building departments, water, lighting, drainage, and heating, all staffed and ready, not to mention extensive grounds, recreation fields and halls, invaluable to the convalescent. The civil general hospitals both in London and the provinces contributed what beds they could spare from civilian requirements; but even the total, added to in some cases by temporary hut wards, was comparatively insignificant and inadequate, while the auxiliary Red Cross hospitals were equipped for convalescents only. Some schools and workhouses were also converted into war hospitals, but not one of those temporary or permanent institutions could compare in all-round suitability with the Asylum War Hospitals; indeed, all that was absolutely essential for the conversion of mental hospitals into war hospitals was additional doctors and nurses.

In a report by medical members of the Board of Control, which is the Government department concerned in the administration of the Lunacy Acts and the welfare of the insane in this country, we should expect to hear how mental disorder occurring in the officers and men of the army was met, not only that directly due to the injuries and miseries of war, but as a result of the ordinary incidence of mental disorder expectancy among three to five millions of men of ages 20 to 50. This incidence was wisely anticipated and provided for by Sir Marriott Cooke and Dr. Bond, and we are not disappointed, for the report fully goes into this question and describes the provision for mental disorders actually due to war conditions, and for those who, actuarially, would have become insane apart altogether from war, but for which latter the army was now responsible.

In Section XVIII of their report the Commissioners give a full and most interesting account of what was accomplished in this direction for the 38,440 patients suffering from psychoneuroses. Most of the Asylum War Hospitals had one or more wards where such cases on discovery were transferred from the general medical and surgical wards of the hospital. This accommodation, however, soon proved inadequate, so a special institution (that known as Moss Side, Maghull, near Liverpool), containing 500 beds, was taken over, but to meet the ever-increasing numbers more and more beds had to be found, for it must be remembered that the mental and physical standards had to be lowered owing to the demands for men from the various fronts, and many men mentally unstable and even actual deficients slipped into the army, only to break down in training or very early on foreign service. So further provision for 250 beds was made at Springfield War Hospital, 300 at the Napsbury War Hospital, 1,000 beds at the Lord Derby Hospital, 600 beds at the Radcliffe War Hospital, 465 beds at the Welsh Metropolitan War Hospital, Cardiff, 400 beds at the Ashurst Hospital, Oxford, and 474 beds at the Maudsley Hospital, which was used as a clearing hospital for London. All these institutions in peace time were mental hospitals, staffed by medical men eminent and experienced in the treatment of mental and allied disorders, such as Sir Frederick Mott, Dr. Edwin Goodall, Dr. Rows, and many others. One has only to read the extremely interesting appendices of the report describing the advanced psycho-therapeutic and other methods of treatment pursued in the mental sections of those war hospitals, to enable one to answer the question what, or where, better arrangements could have been made for those anxious and distressing cases.

We draw attention to this official record of a great achievement partly because the great and humane work amply vindicates itself against much adverse criticism based on misunderstanding, not to say ignorance, and partly because it is gratifying to know what our professional brethren did behind the scenes of war, away from the limelight of the battle front, but also because the value of historical record is much enhanced if we can learn from it solutions of problems of the future. Our readers are aware that bills are being framed by various interested bodies, the Ministry of Health among others, for the early treatment of mental disorders, incipient or actual, without the legal trammels of the Lunacy Acts, the principle underlying all prospective legislation being that as insanity is an illness it should be dealt with by doctors and not by lawyers. Now it is a striking fact that the whole of those 38,440 cases of mental and allied disorders were treated in their early stages—that is to say, for the first twelve months—wholly by doctors without any of the provisions of the Lunacy Acts being invoked at all. True, the cases of the civilian and the soldier are not strictly and wholly comparable; the general disciplinary provisions of the Army Act and King's Regulations ensure that a soldier shall obey the orders of his C.O. and if the C.O. orders his removal to any place the order is obeyed, and so long as he is a serving soldier he may be ordered to the mental ward of a hospital, or to a military mental hospital, if a military medical officer thinks fit. However, the broad fact remains as stated, and we do not find in the report, nor has it been common knowledge that any abuse, injustice, or wrongful detention has arisen. The civilian has no watchful medical officer to procure the best treatment for him without the degrading and often fatal delay of the legal formalities of certification, but he has his family or panel doctor, his friends and relatives, and even himself to take the steps needed to obtain immediately the necessary treatment.

Again, there is an increasing body of opinion that many acute illnesses cannot be treated at home, especially in the case of the poor, and that "team work" (as it is called) in the medical attendance on the case must supplement the individual practitioner. We find in this report evidence of the success of institutional treatment and of the advantages of collaboration by the medical staff, for in all the war hospitals every patient had the advantage of present-day knowledge which could be brought to bear on his case by the specialist and consultant medical staff.

The Commissioners close their report with an expression of indebtedness for the success of their work to the Army Council, the local authorities owning the war hospitals, the commanding officers and staffs of the war hospitals, and last, not least, to the authorities of the "receiving" asylums, without whose co-operation the primary principle of the scheme could not have been carried out.

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FOUNDATIONS OF MEDICINE.

THE address of Dr. Thomas Lewis, which we print elsewhere, sustains a thesis which has been defended by others, but never, we think, in terms that appeal more directly to medical practitioners. The moral of Dr. Lewis was expounded in the Acts of the Apostles. Our medical Peters, confronted with "all manner of four-footed beasts of the earth," and invited to kill and eat, in their turn protest that they have never eaten anything that is common or unclean, and receive the old rebuke, but not in the old spirit. Some conceive a contempt for principles the application of which is so difficult, and become mere empirics; others imagine that the discovery of some method, the mastery of some technique, will provide them with a philosopher's stone. Both errors are tragic enough. The physician of a teaching hospital sees the gradual disillusionment of the youth fresh from the physiological department. The investigator who only meets graduates is impressed by the other fallacy. Having at length won a place in the temple of science for his method—usually after years of neglect or controversy—his is the painful duty of explaining to those anxious to worship at the shrine that the new god, though an exacting master, is not omnipotent.

Dr. Lewis finds a remedy in the humanizing of physiology, and has himself provided illustrations of the process in his special field of applied physiology, while the writings of Dr. Leonard Hill afford many examples of first rate importance. That the proposed reorganization of preliminary and intermediate teaching would have more effect than any other single change is, we think, evident; Dr. Lewis has argued the case so admirably that we need not attempt to reinforce it; instead, we will refer to another weakness of the existing order.

Sir James Paget, in a letter printed in his biography, stigmatized as nonsense the idea that the success of a physician or surgeon "is not only no proof of his being a scientific man, but rather evidence against his being so." And he adds: "Gull has done more thoroughly scientific work, and does still in his study and practice, and shows in all a more scientific tone and power of mind, than a large majority of those who have been elected into the Royal Society as anatomists or physiologists." This censure is not less applicable to our own times. There is, in fact, a touch of intellectual snobbishness in contemporary estimates of scientific merit. That the successful manipulation of delicate apparatus or proficiency in the disciplines of the mathematician or the chemist ought to be recognized by such honours as science has to confer is admitted by everyone. It is not admitted that the man working with the common intellectual tools of all may be in truth performing a task which requires as much ability and gives as much scope for "originality" as anything hedged about by the barbed wire of mathematical or physical technique.

Let us imagine that a general practitioner of medicine set himself the task of carrying on the work that Sydenham and Huxham began, that he were a medical Gilbert White and wrote a natural history of disease as he saw it. Such a task would give scope for the highest powers of intellect and imagination, but it would certainly not be the way to enter the charmed circle of "distinguished scientists." In the past forty years members of the English medical profession—some, general practitioners; some, Government officials; some, medical officers of health—have elucidated the etiology of many industrial diseases. Much that was once obscure regarding, say, industrial lead poisoning, occupational phthisis, the pneumonoconioses, the nystagmus of miners, has now been made clear. The evidence is scattered through official reports, minutes of evidence, and casual articles in our columns and those of other medical journals. Of the authors of these researches what percentage have been crowned with scientific laurels? Had they devoted themselves to laboratory work, how many of them would have been recognized as "eminent scientists"?

We should ourselves stand convicted of intellectual snobbishness if we suggested that the giving or withholding of this or that official honour can turn a true investigator from his path. One does not bribe men to become Pasteurs or Listers with membership of the Academy of Laputa or honorary degrees of the University of Weissnichtwo. But the conventional assumption that certain techniques are scientific and others not does indicate a low general standard of taste which cannot fail to react upon the general level of achievement. There was a time when the experimental philosophers were very definitely regarded as the intellectual inferiors of the erudite bearers of gold-headed canes; Sir George Baker and Sir Lucas Pepys were much more imposing figures in their time than Dr. William Wells or even Dr. Wollaston. The philosophers triumphed, they won their right to an equal share in the governance of the kingdom of scientific medicine, but now the pendulum has swung too far in the opposite direction.

We would preach the doctrine that in the house of science there are many mansions; that the general practitioner of medicine, using no methods not available to all members of the profession, may still achieve scientific results of the highest importance; that the brains with which Opie mixed his colours are still the only indispensable ingredient of scientific research. Dr. Thomas Lewis has realized this important truth and his remarks deserve the earnest attention of all.

In the etiology of disease there are, as Sir James Mackenzie has pointed out, many causes the study of which can only be undertaken by the practitioner; they cannot be isolated and transferred to the laboratory. The rendering available of such knowledge does not depend upon the further loading of the medical armamentarium with special techniques, but upon the inculcation of a higher standard of general mental training. It is our duty to regard the ordinary practitioner of medicine not as a hewer of wood and drawer of water for the superior intellects of the laboratory, not even to incite him to imitate in an amateurish way the particular methods of the laboratory investigator, but to welcome him on equal terms as a scientific investigator and make sure that his achievements do not have to wait longer for recognition nor obtain a less full recognition than those of his colleagues in other branches of scientific work.

THE BOARD OF EDUCATION AND A POST-GRADUATE SCHOOL.

THE letter from Dr. George E. MacLean, the Director in this country of the American University Union in Europe, published at p. 492, deserves the careful consideration of the medical schools of Great Britain, of

London in particular, and also, we venture to think, of the Board of Education. We have reason to believe that the invitations to attend post-graduate courses in Vienna have had a very wide circulation in America and have been accompanied by assurances not only of a cordial welcome, but of plenty of food, comfortable accommodation, and in fact everything needed, exactly as before the war. We do not profess to know how such statements are to be reconciled with the appeals which are being made in this country to save Vienna from starvation, but our interest in the matter lies in the circumstance that American colleagues should be induced to give the medical schools of Great Britain the go-by and travel on to Vienna. Dr. MacLean says that his visitor told him that the British medical profession was not particularly cordial in its reception of American students; this assertion we take leave to deny. But his other statement, that in London the post-graduate work was so submerged and intermingled with the undergraduate work that the opportunities were not comparable with those formerly afforded in Vienna and Berlin, is well founded.

It may be granted that a large part of the attraction Vienna used to have for American and British graduates was that the post-graduate classes included short intensive practical courses in certain specialities during which the teacher laid himself out to help his pupil to make a beginning in the use of special instruments; the stories of the old woman in the class of laryngology who could day by day make her throat more and more difficult to examine are legendary. No attempt has been made to institute such elementary classes in London; it is not a lofty kind of teaching, still it has to be done, and graduates who come for it will stay to attend more scientific courses. London has never succeeded in establishing a post-graduate school on a permanent basis. Several attempts have been made, but when, after the armistice, a need suddenly arose there was no organization in working order. The Fellowship of Medicine gallantly stepped into the breach, and arranged courses in London which admirably met the emergency. It is true that a post-graduate medical association had been brought into existence shortly before the war, but its organization remained rudimentary; the two organizations were amalgamated, and the combined committee has been making a brave struggle to establish post-graduate teaching on an adequate footing in London. We fear that it must be said that the effort has not succeeded. Last autumn the committee recorded its considered opinion that if a post-graduate medical school were to be established in London it was essential that it should have command of a general hospital; for a time there were sanguine hopes that one of the best known general hospitals in London would accept the suggestion. We understand that this hope must now be abandoned, and matters seem to be thrown back very much into the position in which they were at the beginning of 1919; the position is, in fact, more urgent. Not only ought Great Britain to take a legitimate pride in rendering its medical centres attractive to graduates from Great Britain and foreign countries, but the need for post-graduate schools for our own practitioners is, with the ever-extending points of contact between medicine and public authorities, rapidly increasing. From this point of view the situation, in London at least, is discouraging; the medical schools are concerned for undergraduates, and it has not proved easy to arouse sufficient permanent interest in any project for the establishment of a post-graduate school.

That there is a need for a post-graduate medical school, with its own general hospital, in London, has, we understand, been accepted in principle by the University Grants Committee. That Committee, which originated with the Board of Education, but is now dependent on the Treasury, can only make a grant to an existing institution, which must be of university rank. The opportunity of now attracting graduates from beyond the seas is slipping away, and it would be wise to act at once. The matter well deserves careful consideration by the central education authority. We venture to suggest that the Board of Education should at an early date appoint a strong committee, containing representatives of medical teaching and specialists, to report as to the steps which should be taken to fill a serious gap in the existing provision for medical education—a gap to which its medical officer, Sir George Newman, forcibly drew attention in his *Notes on Medical Education in England*, published in 1918.

THE SCIENTIFIC BASIS OF VENTILATION AND OPEN-AIR TREATMENT.

ADVANCES in the practice of medicine may either come empirically or be prompted by scientific research which at the outset may not appear to have any direct practical bearing. It is an old and perhaps somewhat threadbare objection to the whole-time teachers of anatomy and physiology that this plan, though all to the advantage of these sciences, does not in one respect—namely, the application of these sciences to clinical work—compare favourably with the original arrangement by which surgeons and physicians taught these subjects as a parergon to their routine clinical work. Whether or not this holds good, a most valuable field of research has been extended through the establishment by the Medical Research Council of a department of applied physiology under the direction by Dr. Leonard Hill, who has so successfully applied laboratory instruments and results to the problems connected with industry, public health, and disease. In his report to the Medical Research Council on "The Science of Ventilation and Open-air Treatment," a subject that he has long made his own, Dr. Leonard Hill[1] combines the results of much experimental work with a review of existing knowledge, and thus provides in an accessible form a scientific basis for the practice of ventilation and open-air treatment. The ground covered and the store of information, sometimes drawn from unexpected sources, are remarkable.

Successful ventilation is described as depending on the prevention of stagnation of body heat on the one hand, and of uncomfortable chilling of the body on the other hand; but the greater the number of air changes, the less the danger of droplet infection from the carriers of catarrhal disease. Chemical purity of the air is of course important in mines and some factories where poisonous gases or fumes are likely to be liberated, and in rooms into which the products of imperfect combustion are allowed to escape from improperly flued stoves or gas-burners. But, as Dr. Leonard Hill has shown, the widespread explanation of the discomfort and impaired health from crowding in ill-ventilated rooms is erroneous; it is not the excess of CO_2, lack of oxygen, or the presence of organic impurities in the expired air that are responsible; the real cause is not chemical but physical—

[1] The Science of Ventilation and Open-air Treatment. Part I, 1919, pp. 249. Special Report Series, No. 32. Part II, 1920, pp. 295. Special Report Series, No. 52. Medical Research Council. Published by H.M. Stationery Office.

namely, a stagnant, moist, and hot condition of the air. Incidentally Pettenkofer's rule, taught for sixty years by sanitarians, that the percentage of CO_2 is a valuable indicator of the purity of the atmosphere, is controverted; and it is pointed out that the victims of the Black Hole of Calcutta succumbed to heat-stroke and not to suffocation. The supposed presence of organic poisons in expired air has exerted a harmful effect on the progress of hygiene by inducing sanitary engineers to seek after chemical purity and to neglect measures to secure the adequate cooling and evaporative powers of the air, which have a most important influence on health but cannot be estimated by the ordinary thermometer. Accordingly the kata-thermometer—an alcoholic thermometer primarily designed for the measurement of its own rate of cooling when the temperature approximates that of the human body—has been employed as an anemometer and evaporometer in the numerous series of experiments here epitomized. Many observations have been made with this instrument by other workers, and for help in the mathematical and physical aspects of the subject Dr. Leonard Hill is indebted to his assistant, Miss D. Hargood-Ash. The effect of wind on heat loss was thus carefully studied and a mathematical formula constructed.

The penetration and action of light on living organisms, the effect of sunlight, the circumstances in which heat-stroke occurs, the cutaneous pigmentation and racial adaptation to climate, are fully discussed, and many experiments with the kata-thermometer to elucidate evaporation from white and from black skins are recorded. The ideal conditions for health and vigour are realized when the sun warms the skin and breezes keep up a high rate of cooling; it is worth while to quote the epigrammatic dictum: "It is absurd, then, to shut out the sun from rooms in order to save carpets and covers from fading. People who shut out the sun and wind bring about the fading of their own complexions." The chapter "Concerning Clothes" contains the results of ingenious research and practical deductions. The fatal error in clothing is want of ventilation; clothes should be as light as possible, permeable to the air, allow free evaporation, as far as possible not become wet from perspiration, and if damp, not cling to the skin. In ordinary conditions it is only the underfed who suffer from scanty clothing, but the design of aeroplanist or arctic dress should be based on an outer layer of windproof material and inner layers of soft fluffy woollens entangling plenty of air. The loss of health, wealth (50 millions sterling yearly in Great Britain) and happiness due to the smoke nuisance are clearly set out; the remedy is to carbonize all the coal at the gasworks, to use the coke thus provided for generating electric power and lighting, the gas for heating, lighting and power, and to secure the valuable by-products; for this reform gasworks and electric power stations should be run in combination. In the next section, on the dust nuisance, the reasons why some dusts are injurious and others not, are given.

In the chapter on health and environment attention is called to the significant way in which, despite the absolute decline of mortality in both areas, the rural districts maintain almost constantly a relative advantage of more than 30 per cent. over the urban districts in which for more than a generation the sanitary administration has been the more effective of the two. After the picturesque statement, "when the citizen takes his walk in the streets, motor cars pollute the air with exhaust fumes and in dry weather act as dust squirts, making the atmosphere teem with dust particles made unclean by man and animal," Dr. Hill recommends, as a preventive of race decadence, that in a reconstructed England the cities should be small garden towns, smokeless, and provided with well-ventilated workshops and electric light. In the chapter on "Fever and Open-air Treatment" free evaporation from the skin and respiratory tract is considered to be advantageous, probably from the passage of immunizing bodies into the blood with the tissue fluids which make up the volume of the blood; and it is suggested that the old-fashioned treatment of bleeding did good by attracting the tissue juices in large quantities into the blood. The open-air treatment of tuberculosis should be based on physiological principles, and not on a blind belief in the virtues of fresh air. It is just as reasonable to expose a debilitated febrile patient to wet, cold, and windy weather as it is to confine him in an overheated room with stagnant air. While warmth should, if necessary, be supplied, the greatest care should be taken to avoid overheating by the sun's rays. The good effects of the open-air treatment in children and the tuberculous largely depend on the increased metabolism due to the higher rate of cooling induced by the exposure. In the concluding chapter, on the methods of ventilation and heating, Dr. Hill decides that in this climate of mist and cloud the most healthful of the five chief systems is the radiant heat of an open fire and the ventilation maintained thereby and by an open window when necessary.

ROCKY MOUNTAIN SPOTTED FEVER.

ALTHOUGH Rocky Mountain spotted fever is to most of us no more than a name, and that not included in the last edition of the official Nomenclature of Diseases, there are several points of interest in connexion with this disease, which Professor Burt Wolbach[1] has defined as "an acute specific infectious endangiitis, chiefly of the peripheral blood vessels." Except for its geographical qualification, it might be confused with the more or less popular synonyms for cerebro-spinal fever and typhus; and indeed in 1907 Dr. Louis Sambon ingeniously argued that typhus was identical with it. The general rule in nosology is towards analysis rather than towards synthesis and to the effect that diseases tend to be split up into distinct if often allied morbid processes; from an epidemiological standpoint, Hamer duce et auspice Hamer, this has been challenged, and, although bacteriological research provides the surest basis for the differentiation of infections, the recent researches of Noguchi have shown how closely yellow fever and spirochaetosis ictero-haemorrhagica are etiologically related. In Sir William Osler's textbook Rocky Mountain spotted fever is given the synonym of tick fever, and thus confusion might well arise in connexion with the relapsing or tick fever of Uganda and East Africa, which is due to a spirochaete and is probably much the same as the relapsing fever of Europe. Both the diseases are conveyed by ticks, but the nature of the virus of Rocky Mountain spotted fever has been variously described, and has given rise to much discussion. In 1902 L. B. Wilson and W. M. Chowning, who first incriminated the tick as the carrier of the infection, described an intracorpuscular protozoon parasite, a *Piroplasma*, similar to that causing Texas fever in cattle and transmitted by a tick. This was confirmed by J. F. Anderson in 1903, but emphatically refuted by C. W. Stiles the year after. In 1906 H. T. Ricketts, of

[1] *Studies on Rocky Mountain Spotted Fever.* By S. Burt Wolbach, Boston. Reprinted from *Journal of Medical Research*, Boston. 1919, xli, 1–197

the University of Chicago, began his brilliant observations which proved the transmission of the infection by the wood tick (*Dermacentor venustus*), the hereditary passage of the virus through generations of ticks, and complete immunity after an attack of the disease in experimental animals. In 1908 McColla recorded the transmission of the disease by a tick from an infected human being to two volunteers who had incubation periods of three and nine days respectively. Professor B. Wolbach, of the Harvard University Medical School, has now published an exhaustive report on the etiology of the disease, undertaken at the joint request of the Montana Department of Health and the Montana State Board of Entomology, and thus carries Ricketts's work further and confirms it in many respects. He brings forward evidence that the parasite responsible for the disease has three definite morphological types: the initial form in infected ticks is a relatively large extranuclear, delicately staining rod without chromatoid granules; the next is an exceedingly small rod with chromatoid granules, and is seen in nuclei in sections of ticks, and rarely in the smooth muscle cells of blood vessels in mammals; and the final one is a relatively large lanceolate paired form, apparently rich in chromatoid granules—this is the form transmitted from the tick to mammals, and is that found in their blood and lesions; it is probably a resting or slightly more resisting stage. Ricketts recognized this form, but unfortunately confused it with bacteria which are occasionally present in non-infected ticks in large numbers and of size small enough to make error possible. For this parasite the name *Dermacentroxenus* (Dermacentor + ξένος = citizen) *rickettsi*, in honour of Ricketts, who first saw it in the blood, is proposed. Attempts to cultivate it failed, and a decisive conclusion as to its nature—namely, whether it should be classified among the bacteria or protozoa—is not justified by the data at present available. The name Rickettsia was applied by da Rocha-Lima in 1916 to minute bacillary forms found by Hegler and von Prowazek in typhus, and regarded as identical with those described in Mexican typhus by Ricketts; but Wolbach considers that the name Rickettsia is not applicable to the organism that he finds in spotted fever. Typhus and Japanese river fever or flood fever, which present a strong clinical resemblance to it, and to Rocky Mountain spotted fever, will probably be eventually brought into a nosological group characterized by a specificity of the parasite for the peripheral blood vessels. Both in man and experimental animals the lesions of spotted fever are practically confined to the peripheral blood vessels, including those of the external genitals; there is first endothelial proliferation with parasitic invasion of the vessels followed by necrosis of small groups of cells; the affection of the cutaneous vessels accounts for the rash which appears about the third day as a macular eruption, first on the wrists, ankles and back, then all over the body, and becoming petechial. Professor Wolbach, whose monograph contains numerous beautiful illustrations of the organism and of the histological changes, also describes the clinical course of the disease in man, and some other aspects of his research, such as experiments to prove the specificity of the parasite, investigation of its properties, and the features of the infection in experimental animals.

TETANUS IN HOME HOSPITALS.

WE have received from Major-General Sir David Bruce, K.C.B., F.R.S., an analysis of 1,458 cases of tetanus which occurred in the home military hospitals during the years 1914–1918.[1] Summaries of the author's series of separate analyses have appeared from time to time in the BRITISH MEDICAL JOURNAL[2]; in his latest analysis Sir David Bruce, surveying the cases as a whole, has tabulated much information which possesses clinical as well as historical interest. These 1,458 cases correspond to an incidence of 1.2 per 1,000 among 1,242,000 wounded treated in home military hospitals. The occurrence of complete trismus within twenty-four hours was of grave import—seven cases out of ten were fatal; when closure of the jaws occurred after the appearance of other symptoms of tetanus, eight cases out of ten recovered. The mortality for general tetanus without trismus was 24 per cent.; no death occurred among 201 cases of local tetanus. The rate of mortality among the whole series of 1,458 cases was 34.8 per cent., and among all the cases occurring in France and in England 47 per cent.; the death rate in the pre-serum days was about 85 per cent. Doubtless the mortality would have been lower if prophylactic injection in adequate doses had always been employed; many cases of trench feet, for example, succumbed to tetanus before it was learned that they must be treated as wounded men. The death rate was 55.5 per cent. in 1914, 20.5 per cent. in 1917, 26.3 per cent. in 1918, and 15 per cent. in 1919. The mortality was perceptibly less when there was no bony fracture. Figures are given confirming the now well-established finding that the mortality rate varies inversely with the duration of the incubation: in those in whom symptoms of tetanus appeared within ten days of the wound, from the eleventh to the twenty-second day, or after twenty-two days, the rates of mortality were 58.1, 35.3, and 17.3 per cent. respectively. Among the 1,458 cases, 899 (61 per cent.) are noted as having received a prophylactic injection of antitetanic serum. Of these, 203 died, giving a mortality of 22.5 per cent. Among the remaining 559 cases in whom prophylactic treatment, though possibly given, is not recorded, 298 died—giving a mortality of 53.3 per cent. Turning from preventive to curative treatment, Sir David Bruce dismisses the use of carbolic acid, magnesium sulphate, and other salts, with the remark that in home hospitals they proved a failure, and soon dropped out of use. Serum treatment of experimental tetanus in monkeys showed in Professor Sherrington's hands a recovery rate of 44 per cent. in a series of twenty-five animals. Statistical investigation of the value of antitoxin given curatively in the wounded presents many serious difficulties, but Captain Golla, a member of the Tetanus Committee, believes that the therapeutic use of serum is of little or no practical value. Sir David Bruce remarks that "it is clear that medical officers will continue for the present to give a case of tetanus the benefit of the doubt, and use antitetanic serum therapeutically." Antitoxin appears to have no power of neutralizing toxin in the nervous system, but may neutralize free toxin circulating in the blood or lymph. If a fatal amount has not been absorbed by the nerve cells the injection of antitoxin may be of use; by animal experiment it appears to be proved that the intrathecal route is the best.

[1] Reprinted from the *Journal of Hygiene*, vol. xix, No. 1, July 28th, 1920. Cambridge: The University Press.

[2] BRITISH MEDICAL JOURNAL, October 23rd, 1915; June 30th, 1917; September 15th, 1917; December 1st, 1917; February 23rd, 1918; March 16th, 1918; October 12th, 1918; July 5th, 1919.

CARDIO-VASCULAR RESPONSE TO INJECTIONS OF ADRENALIN.

HYPODERMIC injections of adrenalin or epinephrin (to use its non-commercial though perhaps less common name) are so frequently employed as an efficient remedy in asthma, and are now becoming—at any rate in America—such a routine diagnostic procedure in the detection of thyroid intoxication, that information about the reactions thus induced is interesting. Individuals vary much in the cardio-vascular response to these subcutaneous injections, and very unpleasant symptoms, such as palpitation, throbbing in the head, tremors, shivering, nervousness, distress, and precordial pain of an anginoid character, may follow; the physical signs include a rise in systolic pressure, increase of the pulse pressure or the difference between the systolic and diastolic pressures, rapid pulse, mydriasis, and glycosuria. As is well known, Eppinger and Hess, who

introduced the conception of vagotonia and sympathetico-tonia, considered that persons with an excess of adrenalin and increased tonicity of the sympathetic (as distinguished from the autonomic or parasympathetic) system were characterized by a well marked response to adrenalin. This explanation has met with more criticism than confirmation, and Dr. P. W. Clough,[1] in "a study of the cardio-vascular reaction to epinephrin and epinephrin sensitiveness in patients with hypertension" will have none of it. He gave hypodermic injections of 1 c.cm. of Parke, Davis and Co's. 1 in 1,000 adrenalin solution to 95 persons, 32 of whom were regarded as normal subjects, and on the basis of the cardio-vascular response, as judged by the degree of rise in the systolic blood pressure, of the increase in the pulse pressure, and the acceleration of the pulse, divided them into four groups—namely, those with a negative or insignificant reaction, a moderate, a well marked, and a very well marked reaction. Of the two factors—direct stimulation of the heart and vaso-constriction—the first plays the chief part in moderate reactions, whereas in severe reactions the second is also of importance. Out of the 32 normal persons 82 per cent. gave an insignificant or moderate reaction. The most striking point among these observations was the frequency of a well marked reaction in patients with a high blood pressure, which was taken as 155 mm. Hg or more; out of 22 such patients 15, or 68 per cent., were found to show marked reactions to adrenalin, whereas such reactions were obtained in only 26 per cent. of the other 68 cases, and in 18 per cent. of the 32 normal subjects. In none of these 15 cases were there any noteworthy sympatheticotonic symptoms as formulated by Eppinger and Hess. In his critical discussion of the significance of this sensitiveness to adrenalin on the part of persons with a high blood pressure, Dr. Clough, controverting the usual opinion, concludes that it is now well established that adrenalin is not a direct means of maintaining the blood pressure at the normal level, and, further, that in spite of a number of positive results which are shown to be open to fallacy, there is not any satisfactory direct proof of an increased amount of adrenalin in the blood in human disease, or that high blood pressure is due to or associated with adrenal over-activity or "hyperadrenalinaemia." In the great majority of patients with high blood pressure there is no evidence of any thyroid disturbance, and it is therefore doubtful if there is any direct relation between the two conditions. The sensitiveness to adrenalin of patients with a high blood pressure is very probably only one of the manifestations of a general abnormal reactivity of the cardio-vascular system, such patients often showing an unusually well-marked blood pressure response to exercise, emotion, or excitement. These are, of course, the conditions in which Professor Cannon and others believe that an increased secretion of adrenalin occurs, but, as mentioned above, Dr. Clough does not accept this conclusion, and he considers that this sensitiveness may be due to an abnormal state of the sympathetic or the nerve endings without its being necessary to assume over-activity of either the chromaffin system or of the thyroid.

AYERZA'S DISEASE.

Among the papers contributed to the two volumes dedicated to the late Sir William Osler in honour of his 70th birthday there is one on Ayerza's Disease by Professor A. S. Warthin of the University of Michigan, who in a previous paper in 1917[2] referred to this term as first used earlier in that year by Barlaro. As very few British physicians are familiar at any rate with this name, it is perhaps comforting to find that the case reported by Professor Warthin is the first to be published in the English language. The condition is one of chronic cyanosis, dyspnoea, and polycythaemia associated with syphilitic disease of the pulmonary artery; and its clinical manifestations are therefore those of the Osler-Vaquez disease or polycythaemia rubra. Since it was first described in 1892 several forms or varieties of Osler-Vaquez disease have been isolated; thus in 1908 Dr. Parkes Weber recognized a division of the cases into those with and those without splenomegaly, and further those without obvious splenic enlargement have been subdivided into those with increased arterial blood pressure (polycythaemia hypertonica or Geisböck's disease), and those with a normal or low pressure. Congenital cases have been called Blumenthal's disease, and the syndrome, for as such it must be recognized, seems to have a rather unfortunate attraction for eponymous nomenclature. In some cases of primary tuberculosis of the spleen erythraemia has been a prominent feature. Professor Warthin doubts the existence of a primary erythraemia, at any rate with cyanosis and dyspnoea, and believes that in all such cases the erythraemia is compensatory in nature and secondary to pulmonary or other morbid lesions leading to insufficient oxygenation. As an illustration he fully reports a case with the complete symptom- and pathological-complex of the Osler-Vaquez syndrome with chronic cyanosis, high erythraemia, and splenomegaly, with the secondary phenomena of dyspnoea, visual disturbance, gastro-intestinal disorder, general weakness, and oedema in a man who was under observation for five years. *Post-mortem* investigation showed that the underlying cause was a very high grade syphilitic mesarteritis of the pulmonary artery and its branches. The literature of pulmonary arterio-sclerosis is rapidly increasing, particularly in Spanish America, and clinical cases of pulmonary arterio-sclerosis of syphilitic origin appear to be commoner in tropical than in temperate countries. Thus in 1908 Sir Leonard Rogers published in the *Quarterly Journal of Medicine* ten cases of extensive atheroma and dilatation of the pulmonary arteries, almost certainly syphilitic in origin, as a not very rare cause of fatal cardiac disease in Bengal, the principal symptoms being palpitation, dyspnoea, and great dropsy, due to tricuspid regurgitation. Abel Ayerza, Professor of Clinical Medicine in the National University of Buenos Aires, first described in 1901 the syndrome characterized by cyanosis, erythraemia, dyspnoea, cough with mucus or muco-purulent sputum, haemoptysis, headache, vertigo, and somnolence; and in 1913 F. C. Arrillaga gave the most complete account of the condition in a monograph containing eleven cases.

FORGOTTEN COMPRESSES IN THE BLADDER.

AT a recent meeting of the Société Française d'Urologie a communication was read detailing the histories of three cases in which compresses had been left in the bladder. In two patients cystotomy combined with external urethrotomy had been performed for traumatic rupture of the urethra. In the third case an adenoma of the prostate had been enucleated, and the residual cavity plugged. In all three post-operative cystitis was a pronounced symptom, with frequency of micturition and dirty urine. Removal of the foreign bodies, which were found to be encrusted with phosphates, led to rapid cure. Berne-Legarde and Ramos, who recount these experiences, conclude that it is important that the surgeon himself should withdraw the compresses employed, and not delegate the duty to others. An interesting discussion followed, in which several urologists admitted having seen similar cases. Marion has had two, one a prostatectomy performed by himself, the other a similar case operated upon elsewhere. In both he found tampons; one he was able to remove with a lithotrite. Marion has, moreover, on three occasions seen threads from compresses eliminated subsequent to operation. Pillet has met with a case in which phosphates were

[1] P. W. Clough, *Bull. Johns Hopkins Hosp.*, Baltimore, 1920, xxxi, 266-273.
[2] Warthin, *Amer. Journ. Syphilis*, 1917, I.

encrusted on the threads of a pack used in the course of a prostatectomy. Legueu stated that he had in his museum three pieces of gauze which had been overlooked at the end of an operation. As with forgotten swabs elsewhere we have to deal here with three categories: First, those in which a swab intended to serve some temporary purpose or meet some sudden emergency is, in the end, left behind; second, there are those cases in which a swab becomes torn across or leaves behind it fragments shredded off—often through friability engendered by repeated sterilization; third, it occasionally happens that a gauze pack or drain purposely left behind disappears into the wound and is forgotten by the person dressing the case. No one but the operating surgeon can realize how easy it is to leave compresses and swabs behind.

THE CLEANSING OF CHURNS.

THE inquiry made by Stenhouse in 1916[1] showed that between one-hundredth and one-fiftieth of rail-borne milk was sour on arrival at its destination, and that in London alone an annual loss of 90,000 gallons of milk was thus brought about. A similar loss was found to occur in the North of England. Suspecting that inefficient cleansing of milk churns before their return (empty) to farmers might be a source of contamination of fresh milk, Cumming and Mattick,[2] in October and November, 1919, examined 500 empty milk churns found on railway platforms. Only 16 per cent. of the churns examined were clean and dry; 28 per cent., though apparently clean, were wet. In 56 per cent. the presence of milk, or of an evil smell, or of both, was noticed. Each churn was washed out with one litre of sterile normal saline solution and samples of the washings were examined bacteriologically within one hour of collection. Those churns which contained milk, or milky water, showed very high bacterial counts (for example, eighteen million bacteria per cubic centimetre of washings); lactose-fermenting organisms were found in high dilutions. Churns which were apparently clean and dry showed both high and low bacterial counts, but lactose fermenting organisms were few or absent. Out of thirteen clean and dry churns five only showed counts of 5,000 or fewer bacteria per cubic centimetre of washings; the best figure found was 170 bacteria per cubic centimetre of washings, corresponding to at least 170,000 bacteria in the churn. In churns inefficiently cleansed, especially if left damp, a favourable opportunity is afforded for the proliferation of bacteria; the cleansing of such churns presents great difficulties to the farmer, even in the rare instances in which he is provided with the necessary apparatus. The dirtiness of empty milk churns being so great, even in comparatively cold weather, the present inadequate methods of washing and cleansing would appear to contribute in no small degree to the loss of milk caused by souring in transit. In further experiments it is hoped to establish the minimum time of steaming necessary to ensure as complete sterilization of the churns as is possible under practical conditions.

COEXISTING PRIMARY MALIGNANT TUMOURS.

WHEN a patient is known to have a malignant tumour and examination reveals a second neoplasm it is generally the case that the latter is a metastasis of the first. Jentzer[3] reminds us that this is not invariably the case and that two entirely independent neoplasms are occasionally encountered in the same patient. The classification of multiple tumours is thus given by Askanazy: (1) Multiple benign or malignant tumours in the same system; (2) multiple benign or malignant tumours in several systems; (3) multiple metastatic tumours of malignant origin. The second category is that which is rare, and Jentzer has found in the literature only two "triple" cases. The first comprised cancer of the lip, penis, and stomach; the second, cancer of the thyroid, suprarenal hypernephroma, cancer of the gall bladder, polypus of the colon, and adenoma of the pituitary body. To these cases Jentzer is able to add four personal cases. In one man he found a hypernephroma of the left suprarenal with wide metastasis, squamous carcinoma of the tongue and cervical glands, and carcinoma of the stomach. In another, epithelioma of the right eyelid, carcinoma of the stomach, carcinoma of the rectum. In a third, cylindroma of the skin over the knee, carcinoma of the breast. The first case is especially interesting in that each of the three malignant growths was situated in a different and independent system. The author concludes from his experience that it is not absolutely justifiable to give up hope when tumours are discovered which seem to be metastases of one previously discovered (and perhaps already removed); in theory at least, each malignant growth is susceptible of independent treatment. In the last case outlined above both tumours were successfully removed.

[1] *Journal of the Royal Agricultural Society*, lxxviii.
[2] *Journal of Hygiene*, July 28th, 1920.
[3] *Rev. Méd. Suisse Romande*, April, 1920.

THE Harveian Oration before the Royal College of Physicians of London will be delivered by Sir Frederick Andrewes, M.D., F.R.S., Professor of Pathology in the University of London and Pathologist to St. Bartholomew's Hospital, at 4 p.m. on Monday, October 18th.

THE first Murphy Memorial Oration of the American College of Surgeons will be delivered by Sir Berkeley Moynihan on October 11th during a meeting of the College in Montreal. The oration has been founded in honour of the late Dr. J. B. Murphy of Chicago, whose fame as a scientific surgeon and clinical teacher is well known in all schools. On the same occasion Sir Berkeley Moynihan will present to the College a mace, the gift of the surgical consultants of the British Army during the war.

PRESENTATION TO DR. J. A. MACDONALD.

THE following further subscriptions have been received from September 4th to September 20th, in response to the appeal published in the JOURNAL of July 24th (p. 129), towards a presentation to Dr. J. A. Macdonald on the occasion of his retirement from the office of Chairman of Council of the British Medical Association which he had held for ten years. Subscriptions of any amount not exceeding five guineas should be made payable to "The Macdonald Presentation," and sent to the Medical Secretary, British Medical Association, 429, Strand, London, W.C.2.

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Roberts, Dr. K., Shillington, near Hitchin ...	1	1	0
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WE have received from Mr. Robert M. Capon, L.D.S., of Liverpool, copies of a leaflet drawing attention to the importance of clean teeth, and giving instructions as to how and when the teeth should be cleaned. The leaflet (the issue of which had to be suspended during the war) is well suited for distribution to children or hospital patients. The copyright has been presented by the author to the Benevolent Fund of the British Dental Association.

THE MEDICAL DEFENCE UNION.

THE annual general meeting of the Medical Defence Union, Limited, was held at the rooms of the Medical Society of London on September 16th. In moving the adoption of the report of the Council for 1919–20 the President, Sir JOHN TWEEDY, drew attention to the importance of the work carried on by the Union and the advantages of membership of such an organization. He referred to the new arrangement coming into force on January 1st, 1921, by which each member of the Union would be provided with indemnity to the extent of £2,500 against damages or costs of the other side in any case taken up by the Union on his behalf; in his confident opinion the power and influence of the Union would, as a consequence, be greatly increased. He had been impressed during his term of office by the ability of the Council to deal with the great variety of matters—often of extreme difficulty—which came before them, and he had been struck more particularly by the wide acquaintance with the various phases of medical practice which the provincial members possessed. No matter how difficult, complicated, or exceptional the case submitted to the Council, there always seemed to be someone qualified by experience to give a reasoned opinion on it. He believed the Union had an assured future of increasing usefulness before it. The report of the Council and the financial statement for the year 1919 were adopted unanimously.

In the annual report it is stated that during 1919 the membership increased by 127; new members elected numbered 584. The guarantee fund amounted to £11,963. The receipts during 1919 were over £8,000—a record in the history of the Union; the surplus of assets over liabilities was £9,093. The solicitors report that of 138 cases referred to them (compared with 123 during 1918), 31 were libel and slander actions prosecuted or defended; the majority were settled by obtaining full withdrawal and apology from the offenders. During the year 18 malapraxis actions have been defended and 86 miscellaneous and personal matters have been referred to arbitration; the latter phase of the Union's work, though less spectacular than litigation, was in many respects more beneficial. In none of the 138 cases referred to the solicitors was an action lost, and in one instance only, and then in accordance with the express wishes of the member, was it necessary to effect a settlement by monetary payment. In three instances proceedings were taken for the suppression of unqualified practice. In many cases where members had sought to recover payment of their fees through the medium of the courts, the claim—at first resisted on the grounds of wrongful treatment or malapraxis—was admitted as soon as the Union had identified itself with the member in his defence. Members are urged to consult the Union at the earliest possible stage in any matter in which they need assistance; at the time of accepting appointments it is advisable to obtain a written statement of the terms and conditions of the engagement. With regard to complaints against panel practitioners, members are advised that by attending inquiries held before Medical Service Subcommittees they may in many cases avoid the necessity of subsequent appeals to the Ministry of Health. As the result of a number of instances in which reasonable claims for payment have been refused or ignored by the naval or military authorities, the Council feels it necessary to warn medical practitioners that when invited to undertake professional service for the State they should insist upon a definite agreement as to the date and source of payment. By resolution of the annual meeting newly registered practitioners may be admitted to the Union after January 1st, 1921, without entrance fee, provided they join within one year of registration.

ON GETTING RUSTY.

By RUSTICUS.

THE town doctor often expresses envy of the pleasanter conditions in which his brethren in the country do their work. He congratulates us on the fox-hunting, salmon fishing, grouse shooting, and other non-professional slaughter in which we are supposed to pass our time. We, on the other hand, are adept in picturing the heroic circumstances of our lives—our long journeys, always in snow, always at night, to fight with death a battle in which our valour and skill usually gain the victory.

This paper has nothing to do with the physical difficulties and dangers, privileges and pleasures of our lives. Some of the weapons of our warfare are not carnal. How are we to keep them keen and bright?

First, of course, *reading* suggests itself. Reading is necessary—its benefits are obvious. But a man who is tired *by* his work is likely to be tired *of* his work, and it takes some resolution for him to be diligent in professional reading. Resolution carried out, however, soon gathers strength, and professional reading can, in virtue of its own attraction, often win one away from lighter literature. Even then it is not without its danger—the danger of detachment from the concrete subject. The country doctor should check what he reads by what he sees. So far as his own experience carries him he should mentally corroborate or impugn what the writer says, and if his experience is meagre he should use his earliest opportunity to put to the test any proposition suggested. He has not the chance to see all he can read about, but he has the chance to read about most of what he sees. To be profitable his reading should be related to this; I may almost say it should be mainly about this. It is not desirable that he should limit his outlook to the short base-line of his own measuring, but however short it is he should use his own base-line to take the bearings of his distant observations.

I have found it useful to keep one textbook as an index to what I have read on any subject, and to pencil on the margin references to current literature or to other books which I do not possess, or which I use less often. Taylor's *Medicine* is a book which I use for this purpose. I frequently jot on the margins of my own library a name or a date, or both, illustrative of the subject under consideration. I do not insult a writer by scribbling "rot," "bosh," or "splendid." If I have reason to challenge what he says, and if the book is my own, I pencil a quite respectful symbol for future reference. Revisal months or years later has sometimes led me to erase the mark; has sometimes shown me that I had misunderstood the writer; and, in any case, has elicited fuller consideration of the subject.

A means of grace more difficult either to use or abuse is the *laboratory*. This may range from a spoon and a candle to a farm of guinea-pigs. One of my colleagues has an *x*-ray plant; all of us have a few test tubes. I do not think that many general practitioners have time to do their own bacteriological examinations, but, so far as he can, a man will do well to keep his hand in at qualitative and even quantitative examination of urine and blood, even though he depute most of this work to the completely equipped laboratory. If his patients are in good hands, most of his results will be negative, and it is dull work fishing if you catch no fish. Occasionally his own microscope and his own test tube will reward his search.

In reading and in laboratory work it is good to go back to the preliminary studies of our academic curriculum—physics, chemistry, biology, anatomy, physiology. It is wrong to say "go back." I had learned nothing of the orbital structure of the atom, of niton, of Mendelism, of chromaffin tissues, or of vitamines twenty years ago. It is not from books on surgery, medicine, and midwifery that such knowledge as I have got has been gathered. If the fundamental subjects were of use in our undergraduate days they are useful now, and we pilgrims must pitch our old tents on new ground. We cannot even understand new books on medicine, surgery, and midwifery without humble reference to elementary books such as our children are using at school. I confess to having found a good deal that I did not know in Walker's *Inorganic Chemistry* within the last few months. Recently, too, I have become acquainted in many bowls of duckweed with protozoons, rotifers, crustaceans, and diatoms, which formerly I knew only from pictures. "And what the better am I?" I have a more vivid concept of the cilia of the healthy bronchus and of the handicap which the loss of the cilia entails. I understand better the action of vasodilators in flushing the human capillaries, which I never saw in life, and the structure of the myotomes, which I never saw in a human fetus at all. Also, it has been pleasant amusement.

We cannot dispense with such revisal without loss. There are many things which we erroneously think we know. For instance, all of us know in a way that the earth travels round the sun in an ellipse; few of us know that this ellipse, drawn with a blunt pencil on an eight-inch sheet of paper, would be indistinguishable from a compass-drawn circle with the same centre and a radius equal to half of any diameter of the ellipse. Most of us accept the statement that the cornea constitutes about one-sixth of the surface of the eyeball. This is quite wrong; the proportion is about one-fourteenth. The mathematical error is a simple one, prevalent through mere inadvertence.

Is the country doctor never to do any light reading? The paradoxical answer is true that the man who does most hard reading will get through much light reading as well.

Perhaps the most valuable of all prophylactics against mental rust is the *record of work done*. There are many ways of keeping records. That which seems to me ideal is the medical history sheet or card. In the course or at the end of a busy day I should find it impossible to write up every case on its individual sheet. The compromise on which I have declined is a daily journal in which I enter a short note of the condition of each patient and what I have done for him. If it is necessary to review his case at length, it can be done with the help of the dated card index on which the money charges are entered; but I am not now dealing with the *prima facie* use of the journal. Its prophylactic value against rustiness is that when writing it up I have to formulate my concept of the case definitely. Year after year before his death there stood at my shoulder, there still stands, the ghost of my teacher Sir Thomas Fraser checking vague approximation. "Pulse quick! Temperature normal! How quick? What did the thermometer actually record, and where did the patient hold it?" Over and over again has the act of writing down suggested something that I had omitted to investigate, and brought signs and symptoms into significant perspective. I am aware that this would be impossible in many town practices. I have kept it up for about twenty years in a humble class country practice busy enough to save me from bankruptcy.

My urban brother will probably think that I have less opportunity of congenial social intercourse than he enjoys in the town. To a certain extent this is true. It is a great delight to have a talk on any subject with the city physicians and surgeons who honour me with their friendship. But here in the country we discover that the college-bred man has no monopoly of culture. A working shepherd on my list of insured persons has for many years been the Aristotle and the Socrates of relays of brainy lads who have spent their Saturdays in his fields learning botany, astronomy, and honesty of thought from him. He told me that he first read Euclid when he was 28 years old. He noticed from the squared tiling of a dairy floor that "the sum of the first n odd numbers was n^2" (the terms are given as he used them); he correlated this with the binomial theorem, and took me out of my depth. A retired grocer in my village brings his knowledge of Mendelism into practice, and systematizes into theory his results in growing potatoes. And such small music as I know I have learned in large proportion from a working cobbler and a lawyer's clerk.

I am not going to apologize for the size of my hat. As a matter of fact I am not self-satisfied. I feel very rusty. Likewise I know that I am neither the best dressed nor the handsomest man in my neighbourhood. But I should think still worse of myself if I did not take some care for physical and mental decency, if I did not read a little, write a little, wash a little, and shave a little on most days of the year.

England and Wales.

NEW SANATORIUM FOR LIVERPOOL.

OF the tuberculosis sanatoriums contemplated by the National Insurance Act the first to be undertaken was that at Fazakerley, Liverpool. It was begun in 1914, and, but for the war, would have been completed the following year. Now, after many delays, the institution is in working order, and 250 beds are available for the treatment of pulmonary tuberculosis. The institution will be formally opened during the Tuberculosis Conference at Liverpool in October, but the members of the city council were on September 10th, at the invitation of Dr. J. G. Moyles, chairman of the Hospital and Port Sanitary Committee, granted a private view of the buildings and equipments. Unfortunately, Dr. Moyles was prevented by illness from receiving the council, but his place was taken by Dr. Utting, ex-Lord Mayor, and former chairman of the committee. Dr. Utting conducted the party over the institution and grounds, describing the system of working, and giving an interesting history of its evolution. The sanatorium consists of a number of separate buildings, administrative block, nurses' home, male, female, children, and hospital pavilions. The plan was selected by competition, the architects being Messrs. Hooper and Co., of Surrey. The pavilions for adults are two-storied. On the ground floor the front of each compartment can open completely, and behind the compartments runs a corridor the whole length of the building. In the case of the upper story the compartments extend backwards over the corridor—but, being the same size as those below, space is left in front for a roomy balcony. The pavilions for children have only one story. Attached to the institution are workshops, and in the development of the tuberculosis scheme a labour colony is contemplated. Land for this purpose is already available on the corporation's Fazakerley Estate. In accordance with the National Insurance Act, the Treasury has contributed £90 per bed to the initial cost. The Treasury grant amounts to about £22,000 in a total expenditure of over £50,000. The Treasury, in addition, defrays half the expenses and interest on loans. The sanatorium will be in charge of Dr. Rundle, the medical superintendent of the adjoining infectious diseases hospital. The Hospitals Committee, under Dr. Moyles and his predecessor Dr. Utting, are to be congratulated on the completion of this institution for pulmonary tuberculosis, which, with the Highfield Hospital, which has just been taken over from the Select Vestry for the Treatment of Surgical Tuberculosis, should go far to meet the requirements of the city as regards tuberculous disease.

THE ANTI-TUBERCULOSIS CAMPAIGN IN KENT.

With the gradual return of medical officers from military service the depleted staffs of tuberculosis dispensaries, hospitals, and sanatoriums have once more been brought up to the pre-war standard, and in many cases have been considerably increased. The reports of the work done by them in the counties are appearing from time to time, and prove that there is no lack of activity in the endeavour to deal with all the varied problems of prevention and treatment which are now well recognized although by no means solved. From the county council of Kent a good account of the work done during the past year is detailed by Dr. Alfred Greenwood in his annual report. The county is divided into seven districts, each fully staffed, and the experience of each division is recorded separately. It is satisfactory to learn that the ever-present difficulty of dealing with the advanced case shows some signs of being met. The danger of close association with the advanced, and perhaps dying, case—too often unavoidable in small tenements—is more clearly recognized by possible "contacts" than ever before. A special sanatorium for infective cases has been provided, and more active measures for protecting children from possible infection have been adopted. The value of bacteriological work has been abundantly proved, not only for diagnosis but also for treatment of infections other than pure tubercle by inoculation. The want of success in checking the progress of disease is not due to want of means, but simply to lack of power to administer them effectually. No new facts or suggestions have been brought to light, but the previous sum of knowledge has been steadily added to, and former conclusions have been fully confirmed by later experience.

THE METROPOLITAN POLICE.

The annual report for 1918 and 1919 of the Metropolitan Commissioner of Police* has now been issued. In the abstract of the report of Sir Charles Ballance, the chief surgeon, it is stated that the average daily number of members of the Metropolitan Police Force absent through sickness was 834 in 1919, and 906 in 1918, out of about 16,000 men; the daily average loss by sickness was 4.91 per cent. in 1918, compared with 3.42 in 1917. The increased sickness in 1918 was attributable to the influenza epidemic, which reached its maximum on July 7th, when 1,406 men (8.79 per cent.) were on the sick list. During the second wave of the epidemic of 1918 quinine prophylaxis was tried on a large scale, but there was no evidence of its success. The proportion of men who were inoculated with a mixed vaccine was too small for any conclusion to be drawn as to the value of this form of preventive treatment. There were 267 deaths in the Force in 1918, as compared with 100 in 1919, and 107 in 1917. Much more dental treatment appears to have been undertaken

* Report of the Commissioner of Police of the Metropolis for the Years 1918 and 1919. London: His Majesty's Stationery Office. 1920. Price 6d. net.

in 1919, when 7,724 extractions were performed, and 1,076 dentures supplied, compared with 3,860 and 693 respectively in the previous year. From the tabular statement of apprehensions for crime it appears that in 1919, 3.336 per thousand of the estimated population were apprehended for simple or aggravated drunkenness. This figure may be compared with the very low figures of 1.625 and 2.604 for 1918 and 1917 respectively, 4.420 for 1916, and figures ranging, roughly, between 7 and 9 per thousand for 1900 to 1914. Prostitutes apprehended numbered about 2,750 in 1918 and 1919—a slight reduction on the figures of the three preceding years. In the juvenile courts 1,957 cases were charged in 1919 compared with 3,341 in 1918; convictions numbered 466 and 1,008 in these two years respectively; 1,626 children and 983 young persons were received in places of detention in 1919, compared with 2,735 and 1,433 in 1918. The number of persons killed in street accidents in London was 688 in 1919, compared with 612 in the previous year; personal injuries numbered 19,021 in 1919 (the highest figure since 1916), compared with 15,091 in 1918. Of 688 lethal accidents in 1919, 136 were caused by motor omnibuses, 42 by tramcars, 26 by taxicabs, 138 by private motor cars, 25 by motor cycles, and 203 by commercial motors. Among the killed were 20 persons who were improperly riding behind vehicles. Of the 19,027 street accident injuries in 1919, 3,724 were caused by private motor cars and 2,335 by commercial motors.

Ireland.

DEATH OF SIR JOHN BYERS.

WE have received, with very great regret, news of the death of Sir John William Byers, M.D., the distinguished professor of midwifery and of the diseases of women and children in Queen's University, Belfast, and one of the leading members of the medical profession in the North of Ireland. Sir John Byers died on the night of September 20th. We hope to publish an obituary notice in an early issue of the JOURNAL.

DUBLIN DOCTORS AND THE HEALTH COUNCIL REPORT.

A meeting of the Dublin Borough medical practitioners was held in the Royal College of Surgeons on September 9th, when Sir William de C. Wheeler, Vice-President of the Royal College of Surgeons, was in the chair. The principal subject for consideration was the report of the Irish Public Health Council, which was approved; members were urged to procure copies of the report, which are for sale at Messrs. Ponsonby, Ltd., Grafton Street, Dublin (4d. net). Drs. Delaney, E. Magennis, McGrath, Nesbitt, and R. J. Rowlette were appointed to attend the meeting of delegates.

THE LATE MR. ROBERT CAMPBELL.

In our last issue we printed, at page 455, an obituary notice of the late Mr. Robert Campbell, M.B., F.R.C.S., surgeon to the Royal Victoria Hospital, Belfast, who died on September 6th, at the age of 54. The funeral service was conducted at the house in College Gardens, Belfast, by the Rev. H. J. Rossington, who gave an impressive address, and the interment took place in the family burying place at Templepatrick, amid many manifestations of sorrow.

Australia.

[FROM A SPECIAL CORRESPONDENT.]

UNIVERSITIES CONFERENCE.

A CONFERENCE of delegates from all the Australian universities was recently held in Sydney; at one of the sessions a discussion took place on a proposal that the medical course should be extended to six years. It was made by the University of Adelaide, and in supporting it Sir Joseph Verco, the delegate of that university, said that at present the course in Adelaide really extended to six years, as students were able to take certain preliminary scientific subjects outside the university in the first year, and it was thought desirable to bring these first year students into the university. Professor Anderson of Melbourne said the general opinion in Victoria was that the course must be lengthened. Professor Wilson, Dean of the Faculty of Medicine of Sydney, agreed that a change was needed involving the lengthening of the course, but he thought that only one-third of a year might be sufficient. There were some who thought that the students in their fourth and fifth years should have a longer time for clinical work, but he personally thought that the teaching in the early fundamental science was a more important part of the medical training, inasmuch as clinical study could be continued at the post-graduate stage, while a man would, on the other hand, never go back to physics, chemistry, or biology. He was opposed to the idea of pushing these subjects back into the secondary schools curriculum, since science required a more mature mind than that of a youth of 16 or 17 years. He was in favour of the standard for preliminary scientific subjects being raised. It was ultimately decided to refer the whole matter to the Faculties of Medicine in Sydney, Melbourne, and Adelaide.

The conference decided upon the establishment of a Standing Advisory Committee for the Australian Universities. The duties of this committee are:

1. To collect and critically examine information for arrangement in reports, with advice, to the constituent universities.
2. To consider and make recommendations tending towards the co-ordination of educational requirements.
3. To compile a year-book of the Australian universities and to prepare such other publications of common interest as may be requested from time to time by the universities.
4. To arrange for the holding of inter-university conferences and generally to promote the exchange of views between the universities.
5. To act as a medium of communication between the Australian universities and the Universities Bureau of the British Empire and similar bodies in foreign countries.
6. To consider the general aspects of any matter referred to it by any constituent university.
7. Generally to promote common action in any matter concerning the work and development of the universities.

It was decided to refer to this Standing Committee the following questions:

Appointment of graduate-study boards for advice to travelling scholars.
Establishment of uniformity of practice in regard to university emoluments and tenure.
Recognition of Australian matriculation and other academic status by the universities of the United Kingdom.
The relation between the State and the university.

It was also agreed to ask the committee to endeavour to arrange for a preliminary conference in London of the Dominions delegates to the British Congress, and to consider whether it would be advisable to send an Australian Mission to the United States. The conference also decided to make representations to the Government for the alteration of the present duty on scientific apparatus which could not be made in Australia.

RESIGNATION OF PROFESSOR WILSON.

Professor J. T. Wilson, F.R.S., Challis Professor of Anatomy in the University of Sydney, has resigned his chair on his appointment to the Chair of Anatomy at Cambridge. The Senate, on accepting his resignation, passed a resolution expressing their deep regret that the university was to lose the services of one who had been a distinguished teacher for many years, who by his scientific work had brought lustre to the university, and who had been a great inspiration to his students. There is widespread regret at the departure of Professor Wilson, and he will carry with him the best wishes of a very large circle of friends and students.

NEW PROFESSORSHIPS AT THE SYDNEY UNIVERSITY.

At a special meeting of the Senate of the University of Sydney, further consideration was given to the recommendations of the Professorial Board in regard to the McCaughey bequest. Owing to the death of Sir Thomas Anderson Stuart the professorship of physiology is vacant, and on the recommendation of the Faculty of Medicine it was resolved that in future there shall be a Professor of Physiology in charge of the department, and an Associate Professor of Physiology, who would be entrusted with the duties of teaching and research in that branch of the subject of physiology which does not represent the primary

interest of the full professor. A committee was appointed to report upon the steps to be taken to fill the chair of physiology and the associate professorship, the chair of surgery, and the chair of dentistry. It has also been decided to appoint a new professor of English literature, of English language, of French, an associate professor of German and comparative literature, a professor of electrical engineering, and a lecturer in entomology in the Faculty of Science. No steps have yet been taken towards filling the chair of anatomy shortly to be vacated by Professor Wilson.

CHILDREN AND THE SLUMS.

Dr. Harvey Sutton, principal medical officer of the Public Instruction Department of New South Wales, delivered a lecture before the Health Society recently on "The child as the test of social health." He said that one great trend of modern times had been the development of the city, for civilization and city-making had gone hand in hand. In Australia the proportion of persons living in the cities as compared with the country was greater than in the Old World. In New South Wales about two-fifths of the population lived in the metropolis, and the central city was growing at the expense of the country. A study of infantile mortality in this State showed that in 1893 the infantile death rate in Sydney was 140 per 1,000 births, as compared with 97 in the country. After 1898 there was a marked and rapid drop in the rate, and the improvement was remarkable in that the rate for the city now approximated to that for the country. It was noteworthy that the first gain occurred after the city and country milk supplies improved and the provisions of the Dairy Supervision Act of 1895 were enforced. The child problem was the most reliable index of the slum. Overcrowding and associated housing defects showed themselves in the child by dwarfed development, dirt, disease, and death. As a rule the life chances of an infant were twice as good in residential areas as in thickly populated industrial suburbs. The intensity of dental disease was closely parallel to overpopulation. Of 500 children in a suburb with very bad patches of insanitary houses only three children did not require dental attention, whereas ordinarily at least twenty-five would be expected to have perfectly healthy mouths. Dirt, flies and other insects, and facilities for contact, provided every chance for the transmission of infectious diseases. Dwarfed growth was very distinctive of bad housing. The question arose, What should be the minimum size of the house? Each family had a right to adequate breathing space, separation of the boys and girls, facilities for the amenities of a home and for cleanliness. In conclusion Dr. Sutton urged that the importance of the child should be better realized in Australia, where far more attention was given to stock-raising than to the raising of human beings.

Correspondence.

AMERICAN GRADUATES IN EUROPE: LONDON'S OPPORTUNITY.

SIR,—Perhaps your readers will be interested in what was to me a startling item of news. A young qualified physician from one of the Middle Western States of the United States came to my office saying that he was on his way to do some advanced work at Vienna. He said that a committee in Chicago had circularized the physicians in his State saying that they had organized an excursion for physicians to go to Vienna for study, as before the war American physicians were accustomed to do. The committee gave assurances that they had prepared the way for the reception of these physicians in Vienna and for the offering of old-time facilities. Furthermore, the committee were prepared to aid in securing steamer passages and passports. I expressed surprise that any one, in the present famished condition of Vienna and in view of the disruption of the war, should attempt to go there for study. I then presented the facilities for advanced medical study in Great Britain and in London. My acquaintance replied that it was the impression among the physicians in his community that the British medical faculty were not particularly cordial in the reception of American students, and that, despite the fact of the great clinical advantages of London, the post-graduate work was so submerged and intermingled with the undergraduate work that the opportunities were not comparable with those which were formerly enjoyed in Vienna and Berlin.

Inasmuch as it is one of the functions of my office to promote the exchange of American professors and the interchange of migrating students with the British, and to direct here the stream of American students earlier going to the Continent, it seems as though I ought to apprise the medical schools and hospitals of the above fact. If properly organized I believe no better authorities or facilities for post-graduate work exist anywhere than here. But unless something is done at once it is evident that our late enemies will soon recover the patronage and prestige that they had before the war. If the British-American fellowship of our nations is so important to the future peace and progress of the world, as is frequently proclaimed, how important it is that not only our universities but also our great professions co-operate in making personal contacts and professional studies in common. This is a critical and opportune moment. Evidently there is no time to lose. My office, which represents some sixty of the leading universities and colleges of the United States, the Institute of International Education in New York, the American Council on Education, the American Association of University Professors, and the Association of American Colleges, is maintained primarily for the service of American professors and students, particularly graduate students. A cordial welcome has thus far been given by the British to the office and our students, but in the field of medicine it seems to me particularly desirable that the interest already shown by the British brethren should be applied immediately to the further organization and promotion of post-graduate study.—I am, etc.,

GEORGE E. MACLEAN,
Director, British Division, American University Union in Europe.

50, Russell Square, London, W.C.1, Sept. 17th.

THE OCCURRENCE OF URTICARIA AFTER SPECIFIC FEVERS.

SIR,—As I have only just returned home after an absence of several weeks, I have only now had the opportunity of reading Dr. E. Ward's paper on "Urticaria after the specific fevers," which appeared in your issue of August 21st, p. 279. The subject is, in my opinion, deserving of deeper interest than it has hitherto received. The rashes that occur immediately after an attack of a specific fever do not consist solely of urticaria. A blotchy morbilliform erythema and erythema marginatum are not uncommon; and it may be noted that in the first three of the cases recorded by Dr. Ward another rash was observed to be present (blotchy, macular, erythematous). These rashes are well known to those who see much of the acute infectious diseases, and will be found mentioned under the description of "secondary" or "accidental" rashes in most of the special textbooks, and also in works on cutaneous diseases; and more than one writer has reported isolated cases. But so far as I know little has been written concerning their significance. While many of the acute specific fevers exhibit special and characteristic rashes, these secondary rashes are common to all of them.

I believe, with Dr. Ward, that they are a phenomenon of the process of immunization, or perhaps of anaphylaxis. I discussed the question very briefly in an address on "Serum sickness," which was delivered upwards of seven years ago before the Hunterian Society, and was published in the *Lancet* for 1918, i, p. 364.—I am, etc.,

North-Western Hospital, Hampstead, Sept. 18th. E. W. GOODALL.

TREATMENT OF CALCULI IMPACTED IN THE PELVIC PORTION OF THE URETER.

SIR,—In connexion with Mr. Clifford Morson's letter in the JOURNAL of September 18th, p. 454, I should like to ask what evidence he has of the existence of a sphincter at the lower end of the ureter.

Some years ago Mr. T. H. C. Benians and I made a series of careful dissections of this region, and found that instead of there being any increase in the circular muscular fibres at the lower end of the ureter there was a definite decrease. Apparently the way in which regurgitation of urine from

the bladder is prevented is by the interureteral muscular bundle, which forms the base of the trigone, contracting with the muscular fibres of the bladder generally, and so pulling the ureters downwards and inwards towards the middle line, increasing their obliquity, and bringing them within the sphere of control of the bladder fibres, which in the region of the ureteral orifice are partly arranged in a loop-like fashion.

That a considerable amount of movement takes place of a telescopic character between the ureter and bladder is suggested both by the continuation of the interureteral bundle for a considerable distance beyond the bladder, along the posterior wall of the ureter, and by the presence of a very distinct sheath known to German writers as Waldeyer's sheath. We found in certain cases that the portion of the bundle lying along the ureter was almost commensurate in circumferential dimensions with the ureter itself. No doubt the peculiar nature of the flow of the urine into the bladder has something to do with the peculiar anatomical arrangement.—I am, etc.,

WILLIAM WRIGHT.

Medical College, London Hospital, E., Sept. 21st.

PUERPERAL SEPSIS.

SIR,—One wonders whether the biblical quotation from Proverbs, that "In the multitude of councillors there is safety," is really true if it is applied to the divergent views expressed during the last few weeks in the BRITISH MEDICAL JOURNAL as to the causation of puerperal sepsis.

One marvels at the childlike faith of Dr. A. Campbell Stark in his statement that he considers "puerperal sepsis is in *every case* an auto-infection." He has surely never read what I consider overwhelming proof that the variable incidence of this scourge in classified and selected districts is due to the variable incidence in the same districts of septic wounds due to industrial accidents. He will probably be amused at my assertion that in 99 per cent. of cases auto-infection has nothing to do with the case, and that therefore it is unwise for anyone to lay this flattering unction to his soul.

When one considers the meticulous care thought necessary and practised by surgeons to prevent sepsis, why should one wonder that, with the necessarily incomplete methods that have to be adopted by the average general practitioner in puerperal cases, we should so frequently have sepsis as a complication?

Again, I suggest that every medical officer of health will bear me out when I assert that there is a larger percentage of puerperal sepsis among cases attended by both a medical practitioner and a midwife than in cases where the midwife alone is in attendance. It is rather an ugly assertion but it is true, and until this ugly fact is appreciated and acted upon I have little hope of an improvement in puerperal mortality.

If my statistics prove anything they certainly prove that where accidents abound puerperal sepsis also abounds, and the inference is that auto-infection is a bogey which it is unsafe to raise as a determining factor in puerperal sepsis. So convinced am I of this fact that I will forward a free copy of my book on *Puerperal Fever and Allied Infectious Diseases* to any one sufficiently interested; its perusal, I feel sure, will convince an unbiassed reader that the source of puerperal sepsis is not in the patient herself but in her environment, and that in the majority of cases the chief factor is the hands, clothes, or instruments, of those conducting the labour.—I am, etc.,

Heywood, Sept. 14th. GEORGE GEDDES, M.D.

SIR,—The fact that sepsis sometimes follows manipulation during labour is no proof of cause and effect, for it occurs often in cases where there has been no manipulation, and the vast majority of cases where there has been much manipulation are *not* followed by septicaemia. If it be indeed true that manipulation is the chief cause of puerperal sepsis, surely the fact is an opprobrium to obstetrics. What would be thought of a surgeon who refuses to operate because he is not sure of keeping his hands, his instruments, or his patient's skin free from infection?

Yet this is modern teaching: Do not, if you can avoid it, touch a patient in labour, for, if you do, you will very likely give her puerperal fever! Meanwhile the majority of labours in England are conducted by midwives, who are not troubled by such scruples, and whose proportion of septic cases is no greater than that of any other class.

No doubt the ano-perineal region in women always bears organisms from the bowel, but so does that of *everybody* who adopts the usual custom of smearing the part with faecal matter. It is not difficult to get the district surgically clean, and, of the very numerous operations performed on this part of the body, how many are followed by septicaemia?

The theory of auto-infection may be wrong, or, like many generalizations, it may be only part of the truth, but it is the only theory that will explain the present fantastic incidence of puerperal sepsis. It is to be hoped that the new impetus given to the teaching of midwifery may result in some great improvement; but, before any advance is likely to be made, we must clear our minds completely of traditional ideas, derived from the time when antiseptics and bacteriology were unknown.—I am, etc.,

Wanstead Park, Sept. 20th. A. CAMPBELL STARK.

MENINGOCOCCUS CARRIERS.

SIR,—With regard to the question of meningococcus carriers, possibly the following notes may be of interest.

During the early part of last year I was called to a case about 1 a.m. The patient was a boy of 11½ years of age, and when I saw him he was only conscious for short intervals and when spoken to in a loud voice. He showed marked opisthotonos.

The onset of his condition was sudden. He partook of a good tea about 5 o'clock, and afterwards complained of headache and indefinite pains, which gradually increased until he was in the condition in which I saw him. I made a lumbar puncture, withdrawing a quantity of cerebro-spinal fluid, and injected antiserum. Unfortunately the child died about 10 a.m. The cerebro-spinal fluid showed many bacteria, which grew on ordinary blood agar.

The house in which the boy lived with his father, mother, and four other children, was one of a six-tenement house, and was on the ground floor. There were twenty-one inhabitants in the six tenements, from every one of whom I took a naso-pharyngeal swab. From the twenty-one swabs only two were positive. One positive swab was from a sister, aged 10 years (A), the other was from a girl of 10 years (B) who lived in the top flat.

The history of the infection seemed to be as follows: The girl B had been on a visit to Glasgow, where a few cases of cerebro-spinal fever had been reported. She and her mother had returned a fortnight before the fatal case developed. The girl A, sister to the patient, went to school with B, and played with her after school hours.

The conclusion is that B was the original carrier who gave the infection to A, another carrier, who passed it on to the patient.

The case, I think, is interesting, first, from the suddenness of onset and rapid fatal conclusion, and secondly, from the fact that a second carrier intervened between the original infection. The patient, from very careful inquiry, had not, so far as the parents and relatives knew, been in contact with B.—I am, etc.,

Bournemouth, Sept. 20th. J. OLIVER HAMILTON.

MORTALITY OF VENEREAL DISEASE.

SIR,—Mr. Bayly objects to my correction of Sir J. Crichton-Browne's exaggeration. If Mr. Bayly can read plain facts, and if his critical faculty can be divorced from the unscientific melodrama of the "hidden hand of pathology," he may yet see the truth. I showed the gross exaggeration of calling the venereal the third killing disease. As Mr. Bayly wishes to ignore the figures in my letter, he needs to charge me with camouflage, and hastens also to call my case hopeless. His pathological remarks make poor reading, but when anyone in scientific medicine ignores pathology and the duty of proof, he has indeed a poor case. He is the only one who openly funks the demand for pathological proof for their statements, and I expect he has heard of the difficulties of providing them. Dogmatic assertion is so much easier, it goes down with so many audiences—even with many medical ones—for the critical faculty is never prevalent. Lord Astor applied a sensible brake to the alarmists, and

I do not need Mr. Bayly's invitation to collaborate with those who will keep to the truth and avoid the ridiculous and lurid which has disfigured some aspects of a probably beneficial antivenereal campaign.—I am, etc.,

London, W., Sept. 20th. CHARLES RUSS.

POLICE SURGEONS' FEES.

SIR,—As a divisional police surgeon of the county of Wilts, I write to say that I heartily agree with Dr. Withers Green in his remarks on police surgeons' fees (September 11th, p. 413).

The scale of fees for police surgeons of this county was drawn up as far back as 1896, and although the police have received a considerable increase in pay, the fees for their medical officers have not been revised except in the case of payment for examination of recruits. This examination has naturally to be a searching one, and takes considerable time; yet the fee paid was only 2s. 6d. On application to the committee for an increase it was raised to 5s. a head. Surely in these days it is still inadequate. The application for a revision of the other fees which were fixed in 1896 was not acceded to.

It is to be hoped the British Medical Association will take the whole matter up.—I am, etc.,

Devizes, Sept. 13th. R. STEELE.

DOSIMETRIC CHLOROFORM.

SIR,—Dr. Fairlie's article on a simple method of continuing ether administration with the Vernon Harcourt inhaler (BRITISH MEDICAL JOURNAL, September 18th, p. 438) is of interest as advocating the more extensive employment of dosimetric chloroform anaesthesia. Excluding cases of sepsis, where the danger of acidosis precludes its use, chloroform still occupies a valuable place, and its dangers can be, as he remarks, discounted by the employment of a percentage inhaler. Personally, I prefer the one I described in the *Lancet* of April 1st, 1916, as with it there is always an excess of air, rebreathing is impossible, and in addition it is without valves and cannot get out of order. Ether can be substituted after the induction with chloroform by merely pouring in ether to replace the chloroform. With percentage methods I feel that the anaesthetist's motto can indeed be,

"Ademtum tibi jam saxo omnem metum, in aurem utramvis otiose ut dormias."

Many who swear by the drop method have learnt that to laugh at danger means that it comes all the sooner.

Chloroform is by no means disagreeable to take; it is powerful, so that any degree of anaesthesia and relaxation can be produced, and the after-effects are usually slight. Having recently had four doses of my own "smoke," and ether on three occasions, I can honestly as a patient testify to the unpleasant after-results of the latter; I know I was sick for a week after; hence my hope that the present disrepute of chloroform may lessen and that its employment, at least in suitable cases, may again become popular. Anaesthetists should remember that ether is not the only substance to produce sleep. Voltaire, I think, remarks: "Je suis assez semblable aux girouettes, qui ne se fixent que quand elles sont rouilees." The absurd man is he who never changes.—I am, etc.,

London, S.W., Sept. 20th. CHARLES T. W. HIRSCH.

PRICE OF PETROL ON THE CONTINENT.

SIR,—In his article in the JOURNAL of September 18th (page 441) Mr. Massac Buist writes that "in France something over 12s. a gallon is paid for petrol." I think he has been misinformed, or possibly he has mistaken shillings for francs. During a recent tour abroad I found that the price of petrol varied from Fr. 12.50 on the quay at Antwerp to Fr. 13.50 at Lille. The French *bidon* contains 5 litres, which is nearly 8.75 pints, so that the price was a little less than 12½ to 13½ francs per gallon. This is a heavy increase of price for the French to pay, as before the war I can remember buying *essence* round about 2 francs a gallon. But it is a small augmentation for the British tourist; at present he is paying 4s. 3½d. a gallon in England, and the 12½ francs he has to pay in France is, at the present rate of exchange, less than five shillings.

I cannot, therefore, understand why Mr. Buist is satisfied that vendors of motor fuel "would be much in pocket if they ceased selling motor fuel in this country and disposed of all they could secure in markets overseas." I would rather suspect the vendors of being as commercially minded as other gentry of the same ilk, particularly from what a visit to France has taught me in regard to dealers in motor tyres at home. I bought a tyre in devastated Arras for £8 (at the current rate of exchange) of exactly the same size and make for which I had paid £13 in London. The inner tube for it costs £2 1s. 9d. at home and can be bought in France for £1 6s. I can produce the English and French price lists of the same goods, at the same dates. By shoeing my car in France I can save over £20. The difference of £5 on the price of a single tyre seems difficult to explain, and it certainly suggests some profiteering in this country.—I am, etc.,

September 22nd. STCLAIR THOMSON.

Obituary.

SIR WILLIAM BABTIE, V.C., K.C.B., K.C.M.G., LL.D., M.B.,

LIEUTENANT-GENERAL A.M.S. (RET.).

WE announced last week, with deep regret, the sudden and unexpected death at Knocke, on the Belgian coast, of Sir William Babtie, on September 11th. He was found dead in an armchair in his room at the hotel where he had been staying for some time with Lady Babtie and his daughter. The body was brought to London and an inquest was held on September 16th by the Westminster coroner, who found that death was due to natural causes.

William Babtie was the eldest son of the late John Babtie, ex-provost and J.P. of Dumbarton, and was born on May 7th, 1859. He studied medicine at the University of Glasgow, and in 1880 graduated M.B. and obtained the L.R.C.P. and S.Edin. diplomas. In the following year he entered the Army Medical Service as surgeon, and he reached the rank of surgeon-general after thirty years' service. He was senior medical officer in the island of Crete in 1897–98, and for his services during the troubles there was awarded the C.M.G. During the South African war he served as staff officer to the principal medical officer with the Natal army; he was present at the actions leading up to the relief of Ladysmith and the subsequent operations in Natal and the Eastern Transvaal.

Babtie won the V.C. at the battle of Colenso on December 15th, 1899, for his conspicuous bravery in attending wounded under heavy fire and in attempting to save Lord Roberts's son, who was lying in the open desperately wounded. The official record in the *London Gazette* of April 20th, 1900, ran as follows:

The wounded of the 14th and 66th Batteries, Royal Field Artillery, were lying in an advanced donga close in the rear of the guns, without any medical officer to attend to them; and when a message was sent back asking for assistance, Major W. Babtie, R.A.M.C., rode up under a heavy rifle fire, his pony being hit three times. When he arrived at the donga, where the wounded were lying in sheltered corners, he attended to them all, going from place to place exposed to the heavy rifle fire which greeted anyone who showed himself. Later on in the day Major Babtie went out with Captain Congreve to bring in Lieutenant Roberts, who was lying mortally wounded on the veldt (after a gallant attempt to save the guns). This also was under a heavy fire.

A graphic account of how Babtie won the Victoria Cross was contributed to our columns by Sir Frederick Treves on April 28th, 1900, p. 1048. Apart from this gallant act, Babtie's services in the South African war were recognized by mention in dispatches and promotion to lieutenant-colonel, and he received the Queen's medal, with five clasps. In the years that followed he held the appointments of Assistant Director-General A.M.S. at the War Office from 1901 to 1906, Inspector of Medical Services 1907–10, Deputy Director-General A.M.S. 1910–14. In March, 1914, he was appointed Director of Medical Services in India, reaching that country three months before the outbreak of war. He held the appointment until June 5th, 1915, when he was brought to the Mediterranean for special service. In the words of the Mesopotamia Commission, which regarded him, as D.M.S. India, as partly responsible for the breakdown of medical arrangements in the operations for the relief of Kut: "He was considered of sufficient importance to be

detached to Egypt in the spring of 1915 in connexion with medical problems which had arisen there; and in June of the same year need of his ability was experienced elsewhere in connexion with the war, and he was recalled from India." Many felt at the time that the findings of the Commission did less than justice to Babtie, and their view was upheld by the subsequent statement by the Parliamentary Secretary to the War Office that General Babtie's case had been referred to the Army Council, who, after full consideration of all the facts, decided that the explanation he had been called upon to offer was satisfactory in all respects, and that his services should not be lost to the army.

In June, 1915, Sir William Babtie, as has been said, was sent on special duty to act as principal Director of Medical Services in the Mediterranean during the operations in Gallipoli, Salonica, and Egypt. On his return to England in March, 1916, he was made Director, and later Inspector of Medical Services at the War Office. Babtie was honorary surgeon to the King from 1914 to 1919; he was created K.C.M.G. in 1916, and K.C.B. in 1919. On attaining the age of 60 he was placed on the retired list, with the honorary rank of Lieutenant-General. Last year the University of Glasgow conferred on him the LL.D. degree, and at the Annual Representative Meeting he was elected to represent the Army Medical Service upon the Council of the British Medical Association.

Sir William Babtie married in 1903 Edith Mary, daughter of the late W. H. Barry, J.P., of Ballyadam, co. Cork, and widow of Major P. A. Hayes, R.A.M.C. The funeral took place on September 18th at Guildford with full military honours. It was attended by the Director-General of the Army Medical Service and many officers of the R.A.M.C. and representatives of the War Office and the Army Nursing Service. The British Medical Association was represented by Dr. Cecil Lankester, Chairman of the Guildford Division.

Lieut.-General Sir ALFRED KEOGH, G.C.B., G.C.V.O., sends us the following intimate appreciation:

The premature death of Sir William Babtie will have caused widespread regret in the Army, where his high character and qualities were appreciated. If the story of the reform of the Army in matters medical is ever set forth in detail, it will be found that by far the largest part was the work of General Babtie. This is not the time nor the place to give such particulars as are within my own knowledge and experience, but it may at least be stated that after principles of reform had been enunciated, it lay with him, in the various positions which he held in the War Office, to give to them practical shape and workable form. He came to the War Office at a period when a man of his type and experience was the need of the time. It was inevitable that qualities such as he possessed should have marked him as peculiarly fitted for the administrative side of army medical work. Even as a young officer he began to acquire that knowledge of the various branches of the Army which was his special characteristic—a knowledge which in later years enabled him to perform distinguished public service. And yet it is doubtful if this career was the one he would have chosen if left to himself. It was rather that his marked aptitude for business, his passion for accuracy and detail, his peculiarly logical mind, had forced his chiefs to use these rare qualities at a time in the history of the medical corps when such qualities were especially needed. Thus, from almost the very outset of his career we find him in the districts, and later in the commands, involved in administrative work.

I was first associated with him in 1902, when I became Deputy Director-General. He was far from always concurring with the declared policy which at that time was advocated. The admirable frankness which he displayed in expounding his own views was only equalled by the loyalty with which he carried out any definitely detailed policy. It is impossible to speak of this period of Sir William Babtie's career in the restrained words of an obituary contribution. Let me, therefore, say that at a time when, as the phrase goes, many "sat on the fence," awaiting such developments as might determine on which side they would descend, he was never one of them. He was as fearless in the moral as in the physical sphere, propounding his views, too, with such cogency of argument, derived from a complete knowledge of his subject, as to be always convincing. His influence, therefore, in current events during the period of which I speak was very great. Apart from the contentious questions of those years, all now happily settled, in the more prosaic field of routine Babtie has left an enduring mark on the Army Medical Service. He possessed a special knowledge of the history of the Army, and especially of the development of the medical branch. He was essentially philosophic, developing the future on the experience of the past, restraining with rare wisdom by the cogency of his historic proofs temperaments more ardent than his own. It was his peculiar gift to possess this type of mind combined with the spirit of progress, and those who knew him best will recognize him in this poor picture of him which I endeavour to draw.

It is not generally known that Sir William Babtie was the first medical official at the War Office who was concerned with the officer personnel of the Royal Army Medical Corps. Previous to 1902 the head of Branch A.M.D.1 was a civil servant. There was the tradition, still existing elsewhere, that medical men were best governed by men who had had no medical education. It was not without some misgiving that a change was approved. From that time forward it became possible to have some regard to the fitness of the individuals for the posts to which they were deputed, and the labour involved in re-regulating the personnel with some regard to efficiency devolved upon Sir William Babtie. It was inevitable that a task ever fraught with such consequences of good to the soldier should meet with opposition from the more conservative elements in the Corps, but from that time forward the hospitals began to fall into the hands of clinicians, necessary equipment was provided, and full effect given—as it could not otherwise have been given—to the recommendations of the Reorganization Committee and the Advisory Board. With the authority of experts behind him, Babtie was able to establish the era of efficiency ushered in by the events of 1901. This was his great work. Those who know what it involved will best appreciate it. He never failed in anything he undertook.

Of the incidents in his career after my own retirement I am less able to speak; but an examination of the events connected with his few weeks of service in India—a country wholly unprepared for foreign war—and his subsequent service as co-ordinator in the confusion in the Mediterranean, enables me to say that the superlative ability of this great officer was never displayed to more advantage than in the events with which he had to deal. We are left to regret a great public servant, a devoted lover of the Corps of which he was the most distinguished ornament.

Lieut.-General Sir JOHN GOODWIN, K.C.B., Director-General A.M.S., writes:

The death of Sir William Babtie will be deeply felt by every officer in that service to which he had devoted his ability and energy throughout his whole service career. His foremost idea throughout his life was the welfare and progress of the R.A.M.C. and the army nursing services. The more closely one was associated with Sir William the more one realized the sterling qualities which he possessed: his mental ability, his untiring energy, his high ideals with regard to work and duty, and his unswerving loyalty and devotion to the army.

Speaking from a personal point of view, his death leaves a gap which it is impossible to fill; even after his retirement he still retained his energy and intense avidity for work, and the assistance which he so freely gave us on various committees, on the Army Nursing Board, and in many other directions, cannot be expressed in words. The medical and nursing services have lost a loyal friend and helper whose memory will always be held by them in gratitude, esteem, and affection.

Lieut.-Colonel R. H. ELLIOT, Chairman of the Naval and Military Committee, writes:

I gladly and gratefully avail myself of the opportunity to pay a tribute to Sir William Babtie's work on behalf of the Association, and I am sure that all of my colleagues on the Naval and Military Committee will gladly associate themselves with all that I have to say. When in 1919 the Representative Body elected him to the Council in the interests of the R.A.M.C., there were some who felt that one who had held such high positions in the War Office, and who was so closely identified with the aims and

aspirations of his own Service, might not necessarily prove an ideal British Medical Association Councillor. It took but a very short time to break down all such ideas, and at this year's Cambridge Meeting his re-election was felt to be a source of strength to the Association. To those of us who knew him in the intimate atmosphere of committees and subcommittees, his untimely death cannot but leave a sense of grievous loss. The same personal courage that earned him on the field the highest honour that the Sovereign has to bestow was equally conspicuous in debate; but with it there was a geniality of manner, a kindly tact, and an absolute absence of all "side," that made his most outspoken utterances acceptable even to those who disagreed with him. He had an intimate knowledge of the ways of officialdom, and especially of army officialdom; and he was always willing to use his personal influence, and his opportunities of access to those in authority, in the service of the Association. As a member of committee he was invaluable to the chairman, for he never wasted time, or strayed from the subject under discussion. His clear, keen mind went right to the heart of the question, and his ready tongue gave expression to his thoughts in the fewest possible words. He was a member of the Territorial Subcommittee, whom it will be hard, if not impossible, to replace. What he did not know of the subjects discussed was worth forgetting, and every suggestion he made carried the greatest weight with his fellow members. Not the least of his attributes must be reckoned that we one and all not only admired him for the deeds which have made his name a household word and for the ability, of which we were daily conscious, but we also valued very highly his simple, generous friendship. The British Medical Association is the poorer for his loss.

WE regret to have to record the death of Dr. T. F. MCFARLANE, President of the Dundee Branch of the British Medical Association. On September 11th, while mounting the stair of a tramcar, Dr. McFarlane fell, and, landing on the street, sustained a fracture of the base of the skull, from the effects of which he died on September 13th, without in the interval recovering full consciousness. Born in Edinburgh in 1864, Dr. McFarlane studied medicine there and in 1886 graduated M.B., C.M. After holding a house-surgeoncy in Birmingham he joined the late Dr. Gray in Dundee as assistant, and later carried on a full general practice, in which his skill and kindliness endeared him to his patients. Among his colleagues he was noted for his high sense of professional honour and for his *bonhomie* which illumined the earnestness of life by flashes of a somewhat whimsical humour. He was unmarried, and is survived by a brother.

CAPTAIN JOHN WALTER PIGEON, Indian Medical Service, was killed in action in Mesopotamia on September 3rd, aged 33. He was the only son of Dr. Henry Walter Pigeon of Hull. He was educated at Sherborne School, Christ's College, Cambridge, and at St. Bartholomew's Hospital. He graduated as B.C.Camb. in 1914, after taking the M.R.C.S. and L.R.C.P.Lond. in the previous year. He entered the I.M.S. as Lieutenant on August 1st, 1914, passing in first, and became Captain on March 30th, 1915. He had been serving in Mesopotamia for the past five years. He married in 1912, and leaves a widow.

The Services.

DEATHS IN THE SERVICES.

LIEUT.-COLONEL ARTHUR HOLBROOK NOTT, Bengal Medical Service (retired), died suddenly of heart failure at St. Moritz on August 26th, aged 58. He was born on November 28th, 1861, the second son of the late William Nott of Devizes, and was educated at Queen's College, Birmingham, and Durham University, where he graduated as M.B. in 1887 and as M.D. in 1897, having previously taken the M.R.C.S. in 1886 and the L.R.C.P.Lond. in 1887. After filling the post of resident clinical assistant at the Jaffray suburban branch of the Birmingham General Hospital and of resident medical assistant at the Newcastle-on-Tyne Royal Infirmary, he entered the I.M.S. as surgeon on October 1st, 1887, becoming lieutenant-colonel in 1897 and retiring with an extra compensation pension on April 21st, 1919. His first five years' service were spent in military employ, during which he served on the North-West Frontier of India in the two Miranzai campaigns of 1891 (medal with clasp) and in the Zhob Valley expedition of 1890. In June, 1892, he went into civil employ in Bengal, where he served for twenty-three years, latterly holding the important civil surgeoncies of Murshidabad and Howrah. In March, 1915, he was recalled to military duty for service in the recent war and went to Mesopotamia in command of the Bengal ambulance, being mentioned in dispatches in the *London Gazette* of October 19th, 1916.

Colonel Philip Cecil Harcourt Gordon, Army Medical Staff (retired), died on May 4th, aged 56. He was born in London on January 17th, 1864, took the L.R.C.P.and S.Edin. in 1885, and entered the army as surgeon on August 1st of that year, attained the rank of colonel on March 1st, 1915, and retired on September 30th, 1919. He served in the third Burmese war in 1885–87, when he was mentioned in dispatches (*London Gazette*, September 2nd, 1887), receiving the medal with two clasps, and in the South African war in 1901–2, in the operations in the Transvaal, Orange River Colony, and Cape Colony, gaining the Queen's medal with three clasps, as well as in the recent war of 1914–18.

Captain Claud John Burgoyne Pasley, R.A.M.C., died at Port Harcourt, Nigeria, on August 26th. He was the son of the late Dr. C. Burgoyne Pasley of Trinidad, and was educated at the Westminster Hospital, taking the M.R.C.S. and L.R.C.P.Lond. in 1912. After filling the posts of house-surgeon and assistant house-physician at the Westminster Hospital, he joined the West African Medical Staff and served as resident medical officer in the Colonial Hospital at Lagos. He took a temporary commission as lieutenant in the R.A.M.C. on June 15th, 1916, and was promoted to captain after a year's service.

Medical News.

THE Italian Government has conferred upon Dr. Leonard Hill, F.R.S., the Italian silver medal "Ai Benemeriti della Salute Publica."

AT the meeting of the Johnson Society, during the celebrations at Lichfield of the 211th anniversary of the birth of Dr. Samuel Johnson on September 18th, Sir Norman Moore, President of the Royal College of Physicians of London, was installed as president in succession to Sir Sidney Lee.

THE annual dinner of past and present students of Charing Cross Hospital and Medical School will be held in the Adelaide Gallery, Gatti's Restaurant, on Monday, October 4th, at 7 for 7.30 p.m.

AT the opening of the winter session at St. Mary's Hospital Medical School on Friday, October 1st, at 3 p.m., the prizes will be distributed by Dr. J. G. Adami, C.B.E., F.R.S., Vice-Chancellor of the University of Liverpool.

THE opening ceremony of the eighty-sixth winter session of the Middlesex Hospital and Medical School will be held on Friday, October 1st, at 3 p.m., at the Scala Theatre. Professor T. Swale Vincent will deliver an introductory address, and the prizes will be distributed by Admiral of the Fleet, Earl Beatty. After the ceremony tea will be provided at the hospital, and the wards and research departments will be open for inspection. The annual dinner will be held on the same evening at 7.30 o'clock, in the Wharncliffe Rooms, Great Central Hotel.

THE opening ceremony of the winter session at the London (Royal Free Hospital) School of Medicine for Women (University of London) will be held at Hunter Street, Brunswick Square, W.C.1, on Friday, October 1st, 1920, at 3 p.m. The introductory address will be given by Miss A. Maude Royden on "Revolutionary Thought." Academic dress will be worn. A luncheon will be held at the Fishmongers' Hall on Tuesday, October 19th, at which representative women will meet to support the School of Medicine for Women and the development of medical education for women. Sir Alan Anderson, Honorary Treasurer of the school, will be in the chair, supported by Dr. Mary Scharlieb, Miss Aldrich-Blake, Dr. Jane Walker, Lady Barrett, Dr. Louisa Garrett Anderson, and other medical women.

THE autumn session of the West London Post-Graduate College will open on Monday, October 11th, at 5 p.m., when the Right Hon. Sir William Bull, M.P., will give the inaugural address. The chair will be taken by Mr. C. F. Marshall, chairman, West London Hospital. Medical practitioners are cordially invited to attend.

A COURSE of twelve practical demonstrations on the management and feeding of infants and young children will be given by Dr. Eric Pritchard, on Tuesdays and Thursdays, at the St. Marylebone General Dispensary, commencing on October 5th.

SIR ARCHIBALD GARROD, K.C.M.G., F.R.S., has taken up his duties as Regius Professor of Medicine in the University of Oxford, and has gone into residence at Oxford (133, Banbury Road).

A POST-GRADUATE course for 1920–21, to be held in the Hospital for Diseases of the Chest, Golden Square, will commence on October 11th, at 5.30 p.m. The course consists of six lectures, given on Mondays and Thursdays.

THE late Dr. Charles Workman of Glasgow, who held a large interest in the firm of J. and R. Workman of Belfast, left personal estate in the United Kingdom valued at £96,797.

THE price of drugs in Germany has risen enormously, codeine and morphine being quoted at 15,000 marks a kilo, as compared with 290 to 514 marks in 1914.

ACCORDING to the United States National Chamber of Commerce two-thirds of the medical men in the United States possess motor cars.

THE King of Italy has conferred the silver medal for military valour on the Italian Red Cross for its services in the war.

THE Ministry of Health has issued, for the assistance of local authorities and voluntary workers, a list of residential institutions in connexion with maternity and child welfare. The list comprises 116 homes for mothers and babies, with 1,539 beds; 44 homes (including residential nurseries) for children under 5, with 1,053 beds; 88 maternity homes and hospitals, with 1,145 beds; 48 hospitals (including observation wards) for children under 5, with 577 beds; 14 convalescent homes for mothers with babies, with 123 beds; and 8 convalescent homes for children under 5, with 162 beds.

ON September 11th and 12th two new cases of small-pox were admitted to hospital in Glasgow, on September 13th one, on September 14th one (and on the same day the one hundredth death occurred), on the 15th three, on the 16th three, and on the 17th four. Stray cases have also been met with at Larbert, Croy, and elsewhere. The medical officer of health for Glasgow (Dr. A. K. Chalmers) has made a statement regarding the epidemic in which he gives instances in which mild unrecognized cases have been followed by grave cases in the same family; the unrecognized case went about freely and constituted a serious danger. Vaccination, of course, is the means of preventing such dangers, as the sufferers from the mild form of the disease may not themselves realize the nature of their malady. An additional reason for anxiety is the near approach of winter, when the disease is likely to become more infectious and to be more severe in its effects.

Letters, Notes, and Answers.

As, owing to printing difficulties, the JOURNAL *must be sent to press earlier than hitherto, it is essential that communications intended for the current issue should be received by the first post on Tuesday, and lengthy documents on Monday.*

IN order to avoid delay, it is particularly requested that ALL letters on the editorial business of the JOURNAL be addressed to the Editor at the Office of the JOURNAL.

THE postal address of the BRITISH MEDICAL ASSOCIATION and BRITISH MEDICAL JOURNAL is 429, Strand, London, W.C.2. The telegraphic addresses are:

1. EDITOR of the BRITISH MEDICAL JOURNAL, *Aitiology, Westrand, London;* telephone, 2631, Gerrard.
2. FINANCIAL SECRETARY AND BUSINESS MANAGER (Advertisements, etc.), *Articulate, Westrand, London;* telephone, 2630, Gerrard.
3. MEDICAL SECRETARY, *Medisecra, Westrand, London;* telephone, 2634, Gerrard. The address of the Irish Office of the British Medical Association is 16, South Frederick Street, Dublin (telegrams: *Bacillus, Dublin;* telephone, 4737, Dublin), and of the Scottish Office, 6, Rutland Square, Edinburgh (telegrams: *Associate, Edinburgh;* telephone, 4361, Central).

QUERIES AND ANSWERS.

"INCLUDED FETUS."

MR. W. MCADAM ECCLES (St. Bartholomew's Hospital, E.C.1) informs us that he has lately removed an "included fetus" from a male infant aged 8 months. Mr. Eccles would like to hear of any similar cases, and where the specimens are preserved.

CARBON TETRACHLORIDE POISONING.

"A. D. H." writes: I have recently attended a girl who is employed in painting golf-ball moulds. She states that she vomits after every meal and has some headache. She is somewhat anaemic, but otherwise normal. I am informed that the paint used contains carbon tetrachloride, but no other solvent, such as benzol or naphtha. Can you tell me whether any cases are on record of injurious effect of carbon tetrachloride, and, if so, what are the symptoms that have been observed?

*** The vapour of carbon tetrachloride, if frequently inhaled, is injurious to health and has a poisonous effect. Its action is very similar to that of chloroform vapour or the vapour of tetrachloride of ethane, the symptoms of which were described by Dr. W. H. Willcox in the Lettsomian Lectures on jaundice for 1919 (BRITISH MEDICAL JOURNAL, May 17th, 1919). The symptoms at first produced are those of dyspepsia of a toxic type, anaemia, and ultimately a condition of toxic jaundice is likely to develop should the inhalation of the poison continue. Carbon tetrachloride was used as an anaesthetic in the early days after the introduction of chloroform, but owing to its more toxic effects its use for this purpose was discontinued. More recently carbon tetrachloride has been used as a hair shampoo to remove grease, but owing to a fatal case occurring and its dangerous properties its use for this purpose has been discontinued.

INCOME TAX.

"H. H. E. S." fears that if his total income is shown as including income taxed by deduction before it reaches him, he may be assessed upon it and thereby run a risk of paying tax twice on that income.

*** Tax will not be charged in one sum on the total income any more than in the past. Income is still divided into various schedules and classes, and only that income which has not been taxed will be assessed. From the assessments so made will be deducted the allowances to which the individual is entitled, and £225 of the residuum will be charged at 3s.—assuming that the residuum amounts to £225 or more. The advantage of the new system is that in such cases no adjustment has to be made in respect of the income taxed by deduction, the amount of which is often indeterminate until the end of the financial year.

CLIMATE IN RHEUMATOID ARTHRITIS.

"RHEUMATICUS" seeks information as to the climatic treatment of a case of rheumatoid arthritis. The patient's age is 35, and the main features are synovial swelling in both wrists, one knee, and one ankle, with great stiffness in shoulders, back, and knees on rising in the morning. The tendency is to get worse in spite of all treatment—namely, vaccines, spas, etc., in this country (England). Can anything be hoped for from prolonged residence in a warm, dry climate—for example, Egypt—or are there more suitable places than this?

SEBORRHOEA.

"S." writes: I would be very much obliged if any reader of the JOURNAL could tell me of a satisfactory method of treating seborrhoea capitis. The patient, a man aged 40, has used a variety of lotions, etc., including resorcin, sulphur, and salicylic acid. It has existed several years, and is now fast destroying the hair. The scalp is dry, though his skin generally is slightly greasy. He has a rather sensitive skin, has the same trouble in the moustache, and two fair sized patches on the chest and back. He had psoriasis on the back of the hands many years ago, but none since. He consulted a skin specialist, whose only suggestion was the usual sulphur and salicylic acid ointment, which was useless, and very unsightly. Is any internal treatment useful or necessary?

LETTERS, NOTES, ETC.

THE DECIMAL POINT.

IT has frequently been said that the decimal point was first used by Napier of Merchistoun in his book on logarithms (1616), but it was pointed out in an article on Stevinus, the introducer of decimal fractions, in the eleventh edition of the *Encyclopaedia Britannica*, that the point separating the integers from the decimal fractions seems to be the invention of Bartholomaeus Pitiscus. Dr. James Dundas White has told the whole story in *The Times Literary Supplement* (September 9th). It deserves its place as a footnote to the history of science. Pitiscus was a mathematician who in 1600 published at Augsburg a book on trigonometry, containing tables of natural sines calculated to a radius of 100,000. Rheticus, in his *Canon Doctrinae Triangulorum* (Leipsig, 1551), sets out a table of sines, etc., to a radius of 10,000,000. Pitiscus, in his book on trigonometry, published about fifty years later (Augsburg, 1600), gave the sines to radius 100,000—dropping the last two integers given by Rheticus—for example, 38268 34 became 38268. This, however, involved a loss of accuracy, which for certain angles was so serious that when he published a new and enlarged edition in 1608, Pitiscus, while retaining his radius of 100,000, placed an ordinary full-stop—called "full-point" by the printers—after the last integer

and added the subsequent figures (as decimals) after it, thus: 38268.34. Napier did not use the decimal point in the first (Latin) edition of his book published in 1614, but adopted it in the English version published in 1616. When later on the sines were given to radius 1, the whole natural sine was in the decimals, so that 9998942 became 0.9998942. It is worthy of note that this is the proper way to write a decimal fraction; the 0 before the point should never be omitted. In medical writings it very often is, involving a great risk of error, for the full-stop may be omitted by the printer or broken away in printing.

THE USE OF ANTIMONY IN LEPROSY.

DR. UPENDRA NATH BRAHMACHARI, lecturer in medicine at the Campbell Medical School, Calcutta, writes with reference to the note on the use of antimony in leprosy by Dr. Cawston in the JOURNAL of July 17th, 1920, to state that he and his colleague, Dr. Madan Mohan Dutt, published a preliminary note on the treatment of leprosy by antimony in the *Indian Journal of Medicine* for April, 1920. In this note the writers stated that they had treated a few cases of leprosy with intravenous injections of tartar emetic and antimonyl sodium tartrate. In some of the cases there had been a remarkable improvement, especially in the anaesthetic cases. Though they deem it too early to attempt to draw any conclusion, they consider the results encouraging. In the July issue of the same publication Mr. B. B. Maity, house-physician at the Campbell Hospital, stated that he had got very satisfactory results by the intravenous injection of antimony in some cases of leprosy, especially of the anaesthetic variety. Potassium and sodium antimonyl tartrates were used, the dose being from 2 to 4 c.cm. of a 2 per cent. solution. Dr. Brahmachari claims priority in the recommendation of this method of treatment, and hopes to be able at some future time to report some cases in detail.

GALL STONES AND PARSLEY.

DR. C. EDWARDS (Bridport) writes: Parsley is mentioned twice in Celsus (circa B.C. 60), Book II, cap. xxxi: "But these promote urine, whatever things growing in the garden are of good smell, as parsley, rue, etc." Book IV, cap. ix, in the treatment of splenitis: "All things in foods and drinks which are fit for promoting urine. And either the seed of trefoil or cummen or parsley, etc." Dr. J. Carroll (Wigan) might be interested to learn that Dr. Hugh Smythson, in his *British Herbal*, published in 1781, states under Parsley: "The roots are useful in medicine, and are recommended in a strong decoction for the jaundice. It operates powerfully as a diuretic and removes obstructions." His friend Sir John Hill seems to have copied his note from this or some other previous book very literally.

DR. H. FERGIE WOODS (London, W.) writes to say that Hahnemann, a century ago, mentioned parsley as a remedy for gonorrhoea, and says that its value in certain cases of gleet and dysuria has been established.

ERGOT AND IRON MIXTURES.

DR. R. WATSON COUNCELL (London, S.E.) writes with reference to the note on this subject in last week's JOURNAL: The vegetable acids acetic, citric, malic, all discolour iron or steel, leaving a black stain. The best decolorizing agent is phosphoric acid, as will be evident on mixing a usual dose of iron with one of digitalis, diluting the mixture and adding a few drops of ac. phosphor. dil. The mixture clears up at once.

POSTURE IN DEFAECATION.

DR. D. H. VICKERY (Neath) writes: In reply to Dr. Alex. MacRae (September 11th, p. 418), I did not say the *only* cause of hernia and constipation was neglect in not adopting the "squatting posture." My statement was based on my own experience and on common-sense anatomical and mechanical considerations. As to the prevalence of haemorrhoids, I am informed by those qualified to speak that in India this condition is rare owing to the habit of the natives using water instead of paper after defaecation, and hernia is comparatively rare too, the "squatting posture" being the rule.

SKIN ERUPTIONS AND INTERNAL INFECTIONS.

DR. W. J. MIDELTON (Bournemouth) writes: From time to time I have called attention to my belief that germs and toxins may be transferred from internal structures to the skin. I have at present under my care two old ladies suffering from rheumatoid arthritis of many years' duration. I saw and treated them in the early stage by means of multiple acupuncture and application of ol. crotonis co., which is designed to produce a temporary eruption of small pustules on the skin. They both did well, but refused to continue the treatment and sought other advice. By degrees they drifted into a state of great suffering and helplessness, and again appealed to me. For the past four or five years they have been treated symptomatically, chiefly small doses of thyroid extract and other drugs; they have refused the acupuncture method. One of them has suffered severely from attacks of bronchitis, at times assuming an asthmatic character. About six weeks ago severe acute eczema occurred on both arms, and has persisted. This patient is now able to walk fairly briskly from one room to another, and has only a slight cough, with very little dyspnoea or expectoration. A similar coincidence has occurred in other cases. I should like to emphasize here the fact that the eruption produced by the acupuncture and croton-oil method has nothing whatever to do with "sepsis."

AN ABDOMINAL OPERATION IN 1701.

IN the *Glasgow Medical Journal* for September, Dr. George A. Allan gives an interesting account of "Medicine in Scotland in the early part of the eighteenth century." The story is related of Houston, a Glasgow surgeon, who, in 1701, was called to see a woman suffering from abdominal pain and swelling, and from dyspnoea. With a lancet he made an incision of five inches and found a "glutinous substance bung up the orifice." Using lint wrapped round a fir splinter, he managed to remove about nine quarts of this glutinous material, together with a number of cysts the size of oranges. He put in three stitches and dressed the wound with pledgets of wool coloured with balsam, and with napkins steeped in French brandy. The patient lived for thirteen years.

ACUTE ULCERATIVE STOMATITIS CAUSED BY DRUGS.

DR. REDMOND ROCHE (London) writes: Twenty-five years' experience of a fairly extensive use of the coal-tar products inclines me to think idiosyncrasy is comparatively uncommon with regard to them. A few years ago I had an instance of it similar to that recorded by Dr. Bristowe in your issue of September 11th, p. 399. A quarter of an hour after a single dose of antipyrin gr. viij my patient's lips swelled up considerably and the tongue to a lesser degree. The symptoms passed off after a few hours. The patient did not know what he had taken, but told me that a few years previously a doctor in New York gave him a cachet for neuralgia and the same symptoms followed immediately.

TARTAR EMETIC AND ITS TOXICITY.

DR. J. C. MCWALTER (Dublin) writes: Adverting to Dr. Christopherson's suggestion that tartar emetic, given intravenously, appears to be less toxic, perhaps I may state that I administered it intravenously in doses up to 2½ grains without finding any signs of nausea, vomiting, or other indication of inconvenience or toxicity. This was in Malta, where we tried it as a remedy for malaria. I concluded it had little or no curative effect in this disease. In Egypt I tried almost every remedy for bilharziosis without any definite benefit, but we decided that whilst other drugs might be merely useless, the amount of tartar emetic necessary to rid a man of bilharziosis would probably do him harm. I have used it externally in leishmaniasis without any beneficial effect that I could detect.

THE LATE DR. LEONARD GUTHRIE.

MANY friends of the late Dr. Leonard Guthrie at the Hospital for Epilepsy and Paralysis, Maida Vale, London, W., to which he was physician, are anxious to obtain a copy of a photograph of him for reproduction. The photograph will not be injured in any way. Communications should be addressed to Mr. H. W. Burleigh, secretary of the hospital.

A TRIORCHID.

CAPTAIN V. R. SUNDARESA AYZER, I.M.S. (Karachi), sends us photographs and particulars of a case of triorchidism. The condition was discovered accidentally during examination of a 19-year-old native of Trichinopoly. The third testis was situated above the normal left one, and had its own globus major and globus minor, and its own cord, distinctly traceable, at the side of the other normal left cord, as far as the left internal abdominal ring; it was moored to the left wall of the scrotum. The components of its cord were distinguishable, and it appeared to have the normal "testicular sensation."

VACANCIES.

NOTIFICATIONS of offices vacant in universities, medical colleges, and of vacant resident and other appointments at hospitals, will be found at pages 30, 31, 34, 37, and 38 of our advertisement columns, and advertisements as to partnerships, assistantships, and locum tenencies at pages 32, 33, and 34.

THE appointment of certifying factory surgeon at Southall (Middlesex) is vacant.

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